MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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Professor CAYLEY, F.R.S., Vice-President, in the Chair.

The Rev. Daniel Ace, D.D., Laughton Vicarage, Gainsborough;

was balloted for and duly elected a Fellow of the Society.

The Approaching Opposition of Mars. By N. E. Green, Esq.

The Opposition of this planet will occur before the next meeting of the Society. I desire to call the attention of the Fellows to the circumstance that this will be the most favourable opportunity for many years for examining the details of the equatoreal continents.

A careful search should be made for the remarkable dark canals figured by Professor Schiaparelli, which are represented by him as connected with the bays of the sea of Maraldi and the strait of Herschel, especially with the two points of Dawes' Forked Bay.

The northern declination of *Mars* will compensate in great measure for the reduction in its diameter when compared with that of the last opposition, and every use should be made of this occasion for the re-examination of previous drawings.

Extract of a Letter from E. J. Stone, Esq., to the Astronomer Royal, dated Royal Observatory, Cape of Good Hope, 1879, May 5.

I am thankful to say that my Catalogue is at last finished. The last six hours of R.A. will be sent home by the next mail. It will contain 12,450 stars. The work has been most carefully prepared and examined; in fact, I have devoted myself entirely to the work, and have left no stone unturned to make it as good as possible. It must be the chief work of my life. I take with

me my ledgers to read the copy through press, but all the original books of observation and books of reduction will be left here. I shall not require them, for I have read and re-read all the parts not printed. I shall also take with me the ledgers containing the unprinted observations incorporated in the Catalogue, 1877, 1878, and part of 1879. Mr. Gill will therefore be entirely clear from all responsibility for my work. I have pushed on the work here to such an extent that it would have taken more than four years to have printed the observations now completed for press, at the highest pressure ever attained at the Cape. Of course the first thing is to print the Catalogue, which contains all the results. Probably it may afterwards be desirable to print at least the table of instrumental corrections, and the separate mean results for 1877, 1878, and 1879; but I should not think it would be worth the expense of printing the annual Catalogues for these years. In this way expense would be greatly reduced, and everything needful could be given to astronomers. The work as sent home is complete, except that all the constellations have not been put in.

On the Value of the Solar Parallax derived from Observations of Mars made at Ascension Island during the Opposition of 1877.

By D. Gill, Esq.

It is well known to the Fellows of the Society that the special object of the Ascension Expedition was to determine the solar parallax from observations of the diurnal parallax of *Mars*. The reductions are now so far completed that I can communicate the resulting parallax to the Society; and I have thought it better to do so without further delay, as some time must necessarily elapse before the full details can appear in the *Memoirs*.

The general plan of observation and treatment of the reductions has been described in the *Monthly Notices* for November 1878. According to that method, the places of the stars of comparison are regarded as absolutely known.

It was found, however, that the mutual distances of the comparison stars computed from the coordinates determined by meridian observations differed systematically from the same distances measured with the Heliometer according as the fainter star preceded or followed the brighter star. When the brighter star was preceding the computed distance always exceeded the measured distance, and *vice versâ*. In other words (regarding a $4\frac{1}{2}$-mag. star as the mean magnitude of clock stars), the R.A. of the fainter stars appeared to be too great.

In the *Monthly Notices* for December 1878 it was shown that there appeared to be a systematic difference of personal error depending on magnitude, between observations by Eye and Ear

and observations recorded by Chronograph. Separating the results derived from Eye and Ear observations from those derived from chronographic registration, it was found that in the mean the former agreed perfectly with the Heliometer results, whilst the latter showed a still larger discordance, depending on magnitude, nearly proportional to the magnitude, and amounting to nearly $0''{\cdot}25$ of arc per magnitude, for stars from 4·5 to 8·5 magnitude.

Thanks to the energy and kindness of many astronomers, these results have received much independent confirmation.

The most satisfactory method yet followed appears to be to observe the transit of a star over half the wires with the full aperture, then interpose a wire screen and observe the star over the remaining wires; the difference of results, when reduced to the middle wire, is due to Personality. To eliminate error in the wire intervals and possible change of collimation, the operation is repeated in the reverse order—the screen being used with the first wires, the free aperture with the last. The faint diffraction lines formed by the screen are entirely invisible in an illuminated field; the star disk of a 6th-mag. star reduced thus to 8th mag. by a wire screen, is quite indistinguishable from an 8th-mag. star observed in the ordinary way.*

Experiments have been made in this way by Dr. Becker at Berlin, by Professor Lewis Bass at the Dudley Observatory, Albany, U.S., and at Leiden by Professor Bukhuyzen's Assistants, and, I believe, also at other Observatories.

The results, so far as I have received them, entirely confirm the results of the Heliometer observations, and show that the R.A. of faint stars observed by the chronographic method is always too great.

I think it is desirable, before publishing the Memoir on the Parallax in full detail, to wait for the results of observations already made or in progress in connection with this investigation of the effect of magnitude on the R.A.

I have, however, satisfied myself that no alterations which are possible in the adopted places of the comparison stars will produce any sensible effect on the resulting parallax; because, not only must these be very small, but they will be nearly equally distributed amongst morning and evening observations.

I do not, therefore, on that account further delay communication of the parallax obtained. The method of reduction is quite similar with that employed in my preliminary paper (*Monthly Notices* for November 1878), except that two additional terms have been introduced in order to examine the influence of errors in the assumed refraction east and west of the meridian.

If r denotes the tabular refraction for any particular distance,

* I had recently the opportunity to test the experiment myself at Leiden, and I was entirely satisfied as to this point.

then the true refraction, if the observation is made east of the meridian, is supposed to be

$$r+\frac{r}{100}m;$$

and, if the observation is made west of the meridian, is supposed to be

$$r+\frac{r}{100}m'.$$

These terms m and m' are carried throughout the whole computation, so that the final coefficients of m and m' in the result express the effect on the parallax of an error of *one per cent.* in the tabular constant of refraction both east and west of the meridian.

As stated in my preliminary paper, it was obvious, from the arrangement of the order of observation and the positions of the comparison stars, that the final effect of errors in refraction must be insensible, and so the result of rigid computation has proved.

The actual coefficients of m and m', in the final result are

$$-0\cdot0002\,m \qquad -0\cdot0002\,m';$$

so that an error of $2\frac{1}{2}$ per cent. in the refraction tables would be required to change the resulting parallax $\pm0''\cdot001$.

It is therefore impossible to conceive any systematic error due to refraction in its ordinary sense. The Astronomer Royal has, however, called attention to a possible source of error of great importance due to atmospheric dispersion. At considerable altitudes a star is not a disk, but is elongated into a short vertical spectrum by atmospheric dispersion, and we do not see the upper and lower limbs of a planet, but their spectra. It is very easy to conceive that the effect of this may be to shift the apparent place of the planet upwards or downwards, and thus an error in the same sense as parallax may be produced.

Such an effect, however, will vary as the tangent of the zenith distance. Therefore, if we divide the observations of each morning and evening into two groups—observations at greater and smaller zenith distance—we may, by comparing the parallax resulting from the observations at smaller zenith distances with that from observations at greater zenith distances, ascertain whether the effect of atmospheric dispersion affects the parallax in a sensible degree.

Such an investigation accordingly was carried out. The small difference of the two resulting parallaxes ($0''\cdot006$) appears to show that the error due to atmospheric dispersion is insensible.

The observations before and after Opposition were also independently reduced, and the results agree within limits of probable error.

Finally, in order to test whether possible error in the star

places could produce sensible error in the result, a final reduction was made in which only those observations were employed where the same stars were systematically employed the same evening and morning. The result is within the limits of probable error.

The assumed solar parallax was 8″·80.

The following are the direct results, where n is the number of $\frac{1}{100}$ parts which the assumed parallax has to be increased, according to the notation of the Astronomer Royal :—

	Value of n	Weight	Corresponding Solar Hor. Par.
			″
From all observations combined	−0·189	5·20	8·783
„ observations at greater ZD's	−0·164	3·03	8·786
„ „ at smaller ZD's	−0·223	2·17	8·780
„ „ before opposition	−0·098	3·06	8·791
„ „ after „	−0·317	2·14	8·772
„ symmetrical observations only	−0·430	2·12	8·762

The weights given for the various results have been rigidly computed. The probable error corresponding to weight 1 is equivalent to the probable error of a single complete measure of distance.

If then

r = the probable error of 1 observation of distance,

w = the weight of any of the above values of n,

then the probable error of the resulting parallax will be

$$\frac{8{\cdot}8}{100} \times \frac{r}{\sqrt{w}}.$$

Until all means have been exhausted to reduce the errors of the star places, it is is not desirable to compute rigidly the final probable error. But if we adopt even so extreme a probable error as ±0″·40 for the single observation of distance (and there is good evidence that it is less than this), the resulting parallax from all the observations is

$$8''{\cdot}78 \pm 0''{\cdot}015.$$

It is interesting to remark that this result agrees with that derived from the combination of Struve's constant of aberration with Cornu's determination of the velocity of light.

S.S. "Taymouth Castle," Madeira.

Determination of the Longitudes of Berlin, Munich, Leipzig, Vienna, Paris, and Pulkowa.

(*Communicated by Sir G. B. Airy, Astronomer Royal.*)

In the summer of 1876 an expedition was organised by Herr Theodor von Oppolzer for determining the difference of longitude between Greenwich on the one hand and Berlin and Vienna on the other hand. The plan of operations, the selection of instruments, and the course of telegraphic communication were entirely arranged by Mr. Oppolzer. Huts for the instruments and for the galvanic apparatus, piers of masonry, &c., were prepared in the "magnetic ground" of the Royal Observatory, under the sanction of the Treasury Department. Three gentlemen were sent from Vienna to Greenwich, corresponding (I presume) in their observations with three observers at Vienna, and when a first series of observations was terminated (which, under the rigorous conditions of simultaneous observation, occupied a long time), the observers were interchanged. The instruments used for the observations of time were: a transit instrument upon a solid basis, adjusted to the meridian in the usual way; and a transit instrument on a rotatory basis, adjusted so that in its quasi-meridional movement it described a vertical great circle passing through the pole-star. On their chronographs, wire communications, &c., I am unable to speak with perfect accuracy. At Vienna and at Greenwich the difference of longitude of the observing stations was supplemented by geodetic calculations in order to obtain the difference of longitude of the transit instruments of the two Observatories.

Within a few days I have received from Mr. Oppolzer a statement of the final results of the operation. It contains (among other things) a comparison of chronometric with galvanic results. I have pleasure in communicating a translation of Mr. Oppolzer's letter to the Royal Astronomical Society.

G. B. Airy.

1879, *May* 13.

Translation of Letter from Dr. Oppolzer to the Astronomer Royal, dated Vienna, 1879, *May* 1.

I am at last in a position to communicate to you further results for the difference of longitude between Greenwich and Vienna.

Greenwich Transit Circle—Vienna, eastern pier:—

h	m	s	
1	5	21·22	(observations with transit instrument by Nahlik and Kühnert).
1	5	21·23	(observations in the Vertical of the Pole-star by Schram and Anton).

Berlin—Greenwich :—

m	s	
53	34·92	(Nahlik, Kühnert, Becker).

Berlin—Vienna :—

m	s	
11	46·29	(Nahlik, Kühnert, Becker).

Greenwich—Munich :—

m	s	
46	26·29	(Nahlik, Kühnert, Orff).

Munich—Vienna :—

m	s	
18	54·97	(Nahlik, Kühnert, Orff).

The inferences from the partial results give :—

h	m	s	s
1	5	21·23	± 0·02.

For the difference between Pulkowa and Vienna, we have found by the telegraphic method :—

m	s	s
55	57·43	± 0·02.

That between Pulkowa and Greenwich is therefore

h	m	s
2	1	18·66,

which agrees remarkably well with that ($2^h\ 1^m\ 18^s{\cdot}67$) obtained by the Chronometer expedition.

For Paris—Vienna, Löwy and I found :—

m	s
56	0·22.

According to Hilgard, Paris—Greenwich is :—

m	s
9	20·94.

Therefore Vienna—Greenwich would be :—

h	m	s
1	5	21·16,

which agrees well with the above result, assuming the tolerable accuracy of Hilgard's result.

We have, moreover :—

			h	m	s
Greenwich—Bonn, according to		Forsch	0	28	23·31
Bonn—Leipzig	„	Forsch and Bruhns	0	21	10·67
Leipzig—Vienna	„	Oppolzer	0	15	47·18
Therefore, Greenwich—Vienna			1	5	21·16

so that we shall fall into no very great error if we take

h	m	s
1	5	21·21

for the difference of longitude between Greenwich and Vienna.

The reduction of the point of observation to the old University Observatory of Vienna is $+10^{s}\cdot52$. For this latter point, therefore, the difference of longitude with respect to Greenwich amounts to $1^{h}\ 5^{m}\ 31^{s}\cdot73$, which agrees very well with the old (*Nautical Almanac*) value; i.e. $1^{h}\ 5^{m}\ 31^{s}\cdot9$.* In general it would appear that the old longitudes, obtained without telegraphic operations, are not so bad as is commonly supposed.

On the Coincidence of the Bright Lines of the Oxygen Spectrum with Bright Lines in the Solar Spectrum. By Henry Draper, Esq., M.D.

I intend in this paper to speak of the steps that led to the discovery of oxygen in the Sun, to describe very briefly some of the successive improvements of the electrical and optical apparatus employed, and finally to discuss the earlier results and to show their subsequent confirmation.

In 1857, after the meeting of the British Association at Dublin, some of the members, by the kindness of the Earl of Rosse, were invited to visit the 6-foot Reflector at Birr Castle. In this way I enjoyed the advantage of seeing the methods by which that great instrument had been produced, and, on returning to America in 1858, it prompted me to begin the construction of a metallic speculum of $15\frac{1}{2}$ inches aperture. Soon after, by the advice of Sir John Herschel, who had early information of Foucault's work in Paris, the metal was abandoned in favour of silvered glass, and several mirrors were ground and polished. The telescope was constructed especially for photography, and good results were obtained in 1863, culminating in the production of a photograph of the Moon 50 inches in diameter. These were published in the *Smithsonian Contributions to Science* for the succeeding year. The success procured with this instrument prepared the way for making a silvered glass Equatoreal of 28 inches aperture, which was ready for use in 1871, though it has been much modified since. It was obvious that increased light-

* *Note by Mr. Lynn.*—Until 1872 the *Nautical Almanac* has $1^{h}\ 5^{m}\ 31^{s}\cdot9$; from 1873, $1^{h}\ 5^{m}\ 31^{s}\cdot3$, adopted from the *Berliner Astronomisches Jahrbuch* for 1870. The former value was adopted from an investigation by Professor Littrow in 1824 (*Ast. Nach.* vol. iii. p. 64), based on observations of solar eclipses and occultations made about the end of the last century. It is worthy of notice that the result of the mean of six direct comparisons between Greenwich and Vienna there given (from two eclipses of the Sun in 1787 and 1788 and four occultations of stars in *Taurus* and *Gemini* in 1786, 1788, 1791, and 1792) is $1^{h}\ 5^{m}\ 31^{s}\cdot78$, closely agreeing with that of Dr. Oppolzer above.

collecting power and precise equatoreal movements were necessary for the modern applications of physics to astronomy. More recently still there has been attached to the same equatoreal stand an achromatic telescope of 12 inches aperture made by Alvan Clark & Sons, this being particularly intended for solar spectroscopic work.

Soon after the 28-inch Reflector was turned to stellar and planetary photographic spectroscopy, it became evident that the results obtained required for their interpretation photographs of metallic and non-metallic spectra, so that comparisons might be instituted leading to precise knowledge of the elements producing lines at the more refrangible end of the spectrum. This led to a division of the work into two parts, one for the Observatory in the country in the warmer half of the year, the other for my town Laboratory during the winter. It was in the latter that most of the oxygen work has been done, and consequently the engine, the Gramme machine, the induction coil, and the large spectroscope are generally there.

My first photographs of metallic spectra were taken with such apparatus as happened to be at hand, viz. a couple of Bunsen's batteries, an induction coil giving a spark of $\frac{1}{2}$ inch, and a Hofmann's direct-vision spectroscope. The length of the spectrum from G to H was about half an inch, but, though the dimensions were small, the promise was great. After some experiments, however, and after obtaining more powerful instrumental appliances, it seemed best, as able physicists were engaged on the metallic spectra, to turn attention more particularly to photographing the spectra of the non-metals. The exceedingly valuable researches of Dr. Huggins had brought the astronomical importance of nitrogen, carbon, and hydrogen into notice, and these accordingly were next the subject of experiment. Not long after, on examining a series of photographs of the fluted spectrum of nitrogen taken with juxtaposed solar spectra, the suspicion that there was a coincidence of some bright bands in the two spectra was suggested. On pursuing the subject with more and more powerful electrical and optical arrangements, the coincidence of bright lines of oxygen with bright lines in the solar spectrum was discovered.

The original apparatus, as has been said above, was on a very small scale, but it was soon replaced by a larger battery, a 2-inch induction coil, and a direct-vision prism of 1 inch aperture by Browning. The electrical part was made more and more powerful as the research proceeded, the 2-inch induction coil being succeeded by one of 6 inches, and that in turn by a Ruhmkorff coil capable of giving a spark of 17 inches. The battery was eventually superseded by a Gramme dynamo-electric machine which can produce a current powerful enough to give, between carbon points, a light equal to 500 standard candles. When this machine is properly applied to the 17-inch induction coil, it will readily give 1,000 10-inch sparks per

minute. These, being condensed by 14 Leyden jars, communicate an intense incandescence to air, and light enough is produced to permit of the use of a narrow slit, and of a collimator and telescope of long focus.

Since 1877, when the first publication of the discovery of oxygen in the Sun was made, still further improvements, especially in the optical parts, have been completed, so that I am now enabled to photograph the oxygen spectrum with four times the dispersion then employed. For the sake of clearness, it is best to give a brief description: 1st of the electrical part, and 2nd of the optical part.

The electrical part consists of the Gramme machine and its driving engine, the induction coil, the Leyden jars, and the terminal or spark compressor. An advantage the Gramme has over a battery is in the uniformity of the current it gives when an uniform rate of rotation of its bobbin is kept up. Of course this implies the use of a prime mover that is well regulated. The petroleum engine of one-and-a-half horse-power which I have employed is convenient and safe, and does this duty well. As to the Gramme itself, it is only needful to call attention to a modification of the interior connections. In one form the bobbin of wire which revolves between the magnets is double, so that the current produced may be divided into two. Under ordinary circumstances, where the machine is used to produce light, both sides of the bobbin send their currents through the electro-magnets. But if the whole current be sent through a quick-working break-circuit into an induction coil, the electro-magnets do not become sufficiently magnetised to produce any appreciable effect. It is expedient, therefore, to arrange the connections so that one-half of the bobbin gives a continuous current through the electro-magnets and keeps up the intensity of the magnetic field, and then the current from the other half of the bobbin may be used for exterior work, whether continuous or interrupted.

At first a Foucault mercurial interruptor was arranged to make and break the current passing into the primary circuit of the induction coil; but during the past year, by carrying the rate of rotation of the Gramme up to 1,000 per minute, the strength of the current has been so much increased that the mercury was driven violently out of the cup, and hence it was essential to arrange a mechanical break in which solid metal alone was used. This has been accomplished by fastening on the axis of the Gramme bobbin a wheel with an interrupted rim, which serves the purpose well.

As to the induction coil, it is only needful to say that it gives a good thick spark, which is limited to 12 inches, to avoid the risk of injuring the insulation. The Leyden jars are 14 in number, having altogether 7 square feet of coating on each surface.

The arrangement of the terminals from the Leyden jars to get the steadiest and brightest effect has offered great difficulties.

The condensed spark taken in the open air or in a gas under atmospheric pressure pursues, if unconfined, a zigzag course, and this is apt to produce a widening of the lines in the photographed spectrum. But, after many experiments, it turned out that the spark might be compressed between two plates of thick glass, or, better yet, between two plates of soapstone. If the interval between the plates was directed toward the slit of the spectroscope, the lateral flickering of the spark was prevented, and yet at the same time the spark was freely exposed to the slit without the intervention of glass or any substance on which the volatilised metal from the terminals could deposit. Very early in this research it had become apparent that Plücker's tubes could not be employed with electrical currents of more than a certain intensity, partly on account of the deposit that took place in the capillary portion, and partly because the terminals became so hot as to melt and crack the glass. Moreover, it was desirable to use one terminal of iron, so as to be sure that the spectrum of the gas was correctly adjusted to the solar spectrum, and this is impracticable with Plücker's tubes. An additional advantage arises from the soapstone plates, viz. the temperature of the small volume of air between the terminals is materially increased, and increased brightness results. I have tried the effect of warming the air by passing it through a coil of brass tube maintained at a bright red heat, but this does not seem to make any perceptible difference when the terminals are enclosed in the spark compressor.

Spark Compressor.

The optical part of my apparatus has undergone many modifications. At first a Hofmann direct-vision prism was combined with a lens of 6 inches focus; this was soon after replaced by a Browning direct-vision prism and a lens of 18 inches focus, the latter being arranged for conjugate foci, so that it was virtually as if collimating and observing lenses of 36 inches focus were employed. The final system, perfected this winter, consists of a collimator of 2 inches aperture and 26 inches focus, succeeded by two bisulphide of carbon prisms of 2 inches aperture and an observing or photographing lens of 6 feet 6 inches focal length. These prisms belong to Mr. Rutherfurd, and are the same he made for producing his celebrated solar prismatic spectrum. This gives a dispersion of about 8 inches between G and H, and enables me to get original negatives on a scale about half the size of Ångström's charts in the *Spectre Normal du Soleil*. When it is remembered that the light produced by the electric current in the spark compressor is scarcely equal to one standard candle, it will be realised that this great dispersion nearly attains the limit of present possibility. By comparison I have found, when the electric arc from this Gramme volatilises iron, the light is 60 times stronger than the most vivid incandescence of air that I have produced.

The slit of the spectroscope is about one inch long, and opposite the lower half is placed a right-angled prism which serves to bring in a beam of sunlight from a heliostat. We thus have the solar spectrum and the air spectrum upon the plate at the

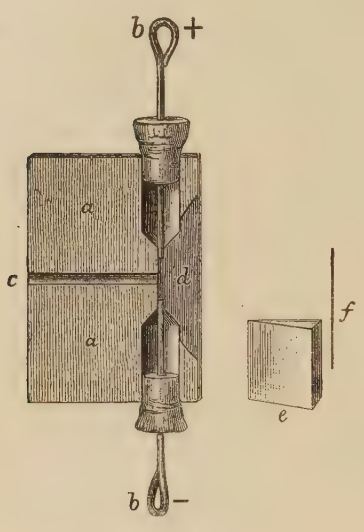


Fig. 1.
Section in plane of narrow opening.

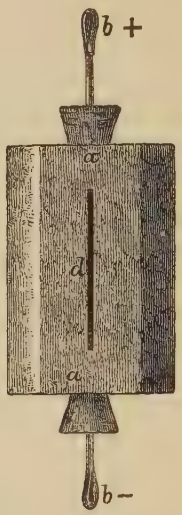


Fig. 2.
Front view.

Spark Compressor.

a a	Soapstone.	*d*	Narrow opening to spark.
b b	Terminals.	*e*	Right-angled prism.
c	Aperture for introducing gases.	*f*	Slit of spectroscope.

same time, so that the two spectra on the negative are, strictly speaking, simultaneously produced. Moreover, by the aid of a magnifier we can ascertain, just previous to an exposure, whether the adjustments are in the best order. It is not commonly known that, to obtain the last degree of exactness in coincidence between a solar and an air spectrum, many precautions are necessary; and for this reason it is desirable to have iron vapour present at one of the poles, so as to determine the reliability of the coincidence by comparing iron in the spark spectrum with iron in the Sun.

Diagram of Photographic Spectroscope.

Having thus alluded to some of the principal peculiarities of the apparatus constructed for this research, it is proper in the next place to point out the nature of the evidence afforded by the photographs of the presence of oxygen in the Sun. The first

photographs were on so small a scale that they did not even give rise to a suspicion of this fact, and it was not until 1876 that I felt sufficiently sure to make any publication. At this time the original negatives were about 2 inches long from G to H, and they bore an enlargement of three or four times quite well. The Albertype printed in 1877 in *Nature*, the *Comptes Rendus*, and the *American Journal of Science* was produced from such

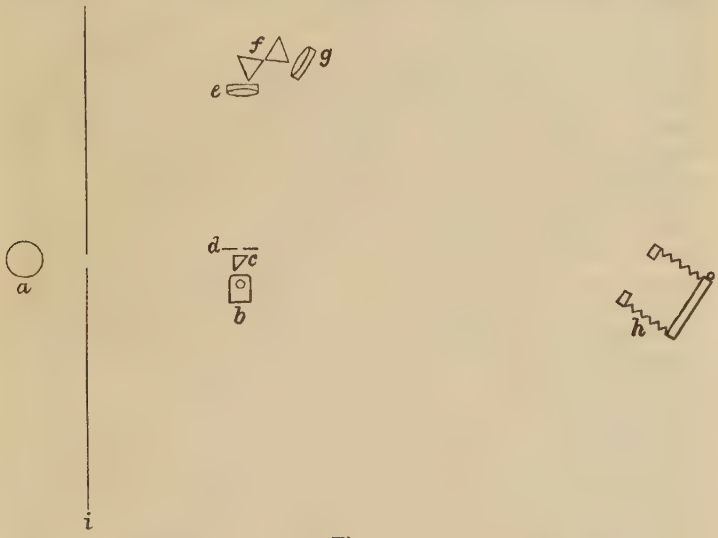


Fig. 3.

a	Heliostat Mirror.		Two bisulphide prisms.
b	Spark compressor.	*g*	Photographic objective.
c	Right-angled prism.	*h*	Camera.
d	Slit.	*i*	Window shutter.
e	Collimator.		

an enlargement. Since that time, in order to meet the criticism that perhaps the dispersion was not sufficient to disclose the lack of coincidence if such existed, I have increased the dispersion four times and am thus enabled to make enlarged photographs on a scale about twice the size of Ångström's chart. Enlargements of the juxtaposed spectra of air and Sun on this scale are now presented for inspection.*

Of course an enlargement never does justice to the original from which it was produced; and, in order to study the matter faithfully, the negative must be examined carefully with a magnifier. Beside this, owing partly to the fact that the solar spectrum has suffered from absorptive influences, both in the Earth's atmosphere and in the solar atmosphere, the conditions under which the oxygen spectrum is seen when compared with the spark spectrum are modified. In fact, a critical study of

* These Enlargements were exhibited at the Meeting.—Ed.

the two spectra demands that each line of oxygen should be separately photographed with the corresponding region of the Sun's spectrum, so as to reproduce as nearly as possible the same conditions for each. As an instance of the modifications which may be caused by the solar atmosphere, the superposition of absorption lines on the bright lines of oxygen may be mentioned. If, as seems to be the case, the stratum giving the oxygen spectrum in the Sun lies deeper than the reversing layer in which iron exists, I see no reason why an iron absorption line, for instance, may not fall upon an oxygen bright band. In support of this supposition that oxygen is photospheric, it may be stated that, though I have examined the chromosphere on many occasions, I have not as yet seen the bright oxygen lines project beyond the apparent limb of the Sun as observed in the spectroscope, although several of the chromosphere lines catalogued by Young were readily visible. On consulting with Professor Young, he expressed the opinion that the oxygen groups near G did not appear as bright lines in the chromosphere, even under the exceptionally favourable circumstances he enjoyed at Sherman. For the purpose of continuing the study of this point, and also of examining small areas on the Sun, faculæ, spots, &c., Alvan Clark & Sons are constructing for me a special spectroscope, which can give the dispersion of twenty heavy flint prisms and can bear high magnifying power.

If it be conceded that there are bright lines in the spectrum of the solar disk, and this seems to be the opinion of several physicists, especially Lockyer, Cornu, and Hennessy, the question of their origin naturally attracts attention. It seems that there is great probability, from general chemical reasons, that a number of the non-metals may exist in the Sun. The obvious continuation of this research is in that direction. But the subject is surrounded by exceedingly great obstacles, arising principally from the difficulty of matching the conditions as to temperature, pressure, &c. found in the Sun. Anyone who has studied nitrogen, sulphur, or carbon, and has observed the manner in which the spectrum changes by variations of heat and pressure, will realise that it is well-nigh impossible to hit upon the exact conditions under which such bodies exist at the level of the photosphere. The fact that oxygen, within a certain range of variation, suffers less change than others of the non-metals has been the secret of its detection in the Sun. It appears to have a greater stability of constitution, though Schuster has shown that its spectrum may be made to vary. I have already begun an extended series of experiments on the non-metals; but the results exhibit such confusion that their bearing cannot at present be distinctly seen. In the case of nitrogen the broad bands between G and H exhibit, under the most intense incandescence, a tendency to condense into narrow bands or lines, and indeed there are some sharp lines of nitrogen in the photographs now presented.

It does not follow, therefore, that the bright bands of oxygen are necessarily the brightest parts of the solar spectrum. Other substances may produce lines or bands of greater brilliancy.

There is also another cause for a difference of appearance in a bright-line spectrum produced in a laboratory and bright lines in the Sun. While the edges of a band in the spark spectrum may be nebulous or shaded off, the corresponding band in the solar spectrum may have its edges sharpened by the action of adjacent dark lines due to one or another of the metallic substances in the Sun.

On the whole, it does not seem improper for me to take the ground that, having shown by photographs that the bright lines of the oxygen spark spectrum all fall opposite bright portions of the solar spectrum, I have established the probability of the existence of oxygen in the Sun. Causes that can modify in some measure the character of the bright bands of the solar spectrum obviously exist in the Sun, and these, it may be inferred, exert influence enough to account for such minor differences as may be detected.

In closing, it may be well to give some idea of the amount of labour and time this research has already consumed, and this cannot be better done than by a statement of the production of electrical action that has been necessary. Each photograph demands an exposure of 15 minutes, and, with preparation and development, at least half an hour is needed. The making of a photograph, exclusive of intermediate trials, requires, therefore, about 30,000 10-inch sparks, that is 30,000 revolutions of the bobbin of the Gramme machine. In the last three years the Gramme has made 20 millions of revolutions. The petroleum engine only consumes a couple of drops of oil at each stroke, and yet it has used up about 150 gallons. Each drop of oil produces two or three 10-inch sparks. It must also be borne in mind that comparison spectra can only be made when the Sun is shining, and clouds therefore are a fertile source of loss of time.

London, June 10, 1879.

On the Photographic Semi-diameter of the Moon.

By Professor C. Pritchard.

In the course of measuring the Lunar Photographs which have been obtained in the Oxford University Observatory by means of the De La Rue Reflector of 13 inches aperture, it occurred to me, for the purposes simply of acquiring confidence or otherwise in the laborious processes which have been undertaken, to compare the mean semi-diameter of the Moon thereby obtained with the mean semi-diameter adopted in the *Nautical Almanac*; and also to compare the photographic results with

similar results deduced from Wichmann's measures made with the Königsberg Heliometer in 1844.

The process of computation adopted at Oxford is very nearly the same as that proposed by Bessel and employed by Wichmann. Two small but well-defined craters were selected on the photographs, viz. *Ptolemy*, A, and *Triesnecker*, B; the latter being purposely chosen on account of its close proximity to the mean centre of the Moon's disk. A few measures have also been taken with *Hypatia*, B.

Distances from each of these craters were measured to six or seven points on the Moon's limb, arbitrarily but conveniently selected, and subtending known measured angles at the particular crater. These distances and angles were measured by means of Mr. De La Rue's engine, the scales and screws of which had been previously examined with scrupulous care. By methods well known to astronomers these data afford equations of condition for the determination of the photographic diameter of the Moon, as seen at the moment from the place of observation.

From each of these local and apparent semi-diameters, the mean geocentric diameter of the Moon has been computed by means of the formula

$$\Delta = \frac{\sin \varpi}{\sin p} \cdot D,$$

where D is the geocentric semi-diameter at the time of observation, p the corresponding parallax, Δ the geocentric semi-diameter at the mean distance, and ϖ the mean parallax. Twenty of these determinations thus obtained are tabulated below, and are printed in column II.

In column III. are printed the differences of the several values in column II. from the arithmetic mean (15′ 34″·175) given at the bottom of the column; the examination of these residuals by well-known methods shows that the probable error of the mean diameter of the Moon thus obtained is ±0″·069. The mean geocentric semi-diameter in use in the *Nautical Almanac* appears to be 15′ 34″·10; and the remarkably close agreement between these two results, thus obtained by methods entirely independent of each other, warrants, I think, confident reliance on the accuracy of the photographic method.

It is, however, to be carefully noticed that, while this preliminary and partial investigation may, it is hoped, attract the interest of the Society to a novel but most careful application of photography, nothing beyond this general accuracy of the method is here sought to be established.

With regard to Wichmann's measures, it may be remarked that the instrument he used was the Königsberg Heliometer, and that he reduced his resulting measures by nearly the same methods as those adopted at Oxford. In column IV. I have given the resulting differences between the local semi-diameters

thus computed and the corresponding ones given in the *Berliner Jahrbuch*, in which Ephemeris Burckhardt's values are adopted. The probable error of the mean of twenty consecutive semi-diameters is about $\pm 0''\cdot13$, being nearly double that which attaches to the photographic results.

From a remark found in Wichmann's most able paper, it appears that the mean value of the Moon's semi-diameter as obtained from the whole series of his measures, fifty in number, differs from that obtained from his twenty consecutive measures, by a quantity far too inconsiderable to be taken into account.

The Heliometer having hitherto been considered the most accurate instrument for the measurement of comparatively small distances, it seems desirable to place the results of the heliometric and the photographic methods in juxtaposition.

Column I. Observed Crater.	Column II. Moon's Mean semi-diameter.		Column III. Difference from Arithmetic Mean (15′ 34″·175).	Column IV. *Berliner Jahrbuch* semi-diameter 1844 − Heliometer measure.
	′	″	″	″
Triesnecker, B.	15	34·295	0·120	−0·164
Ptolemy, A.	15	33·931	0·244	−0·907
Triesnecker, B.	15	34·820	0·645	+0·013
	15	34·315	0·140	−0·854
	15	34·568	0·393	−1·132
Ptolemy, A.	15	34·150	0·025	−1·457
Triesnecker, B.	15	33·457	0·718	−1·196
	15	34·031	0·144	−0·511
Ptolemy, A.	15	34·591	0·416	−0·453
Triesnecker, B.	15	34·380	0·205	−1·454
Ptolemy, A.	15	34·801	0·626	−0·878
	15	34·787	0·612	−3·332
	15	33·884	0·291	−2·475
Hypatia, B.	15	34·262	0·087	−1·080
	15	33·590	0·585	−0·694
	15	34·178	0·003	−0·242
	15	34·271	0·096	−0·536
	15	33·280	0·895	−2·294
	15	33·469	0·706	−1·849
Ptolemy, A.	15	34·447	0·272	−0·252

The mean of the above photographic semi-diameters is 15′ 34″·175, with a probable error of $\pm 0''\cdot069$. The comparison of columns III. and IV. will furnish an idea of the results which may be expected from the two methods of measurement. Further details will be given in the next fasciculus of the Oxford University Observatory, to be published at an early date in next year.

M M

Spectroscopic Observations of the Motion of Stars in the line of Sight, made at the Temple Observatory, Rugby.

By Geo. M. Seabroke, Esq.

For some time past I have been engaged, whenever time would allow, in the spectroscopic estimation of the motion of stars towards and away from our system. The results I have obtained are very inconclusive; but I think it desirable, for the following reasons, that I should bring them before the notice of the Society without further delay. First, they are all *bonâ fide* attempts at correct measures and were carefully made, and a perusal of them, together with those of other observers, will show the wide difference of the results on the same star; and will prevent hasty conclusions based on the assumed accuracy of such measures, while they still show sufficient accordance to give some hope that, with increased instrumental power, accurate results may be obtained. Secondly, this paper is a contribution to the knowledge of the best methods of making delicate observations of this kind, and an account of early attempts shows what to avoid as well as what to do.

The instrument I have used in this research is a Newtonian Reflector of $12\frac{1}{8}$ inches aperture and 6 feet 6 inches focal length, mounted equatoreally, and a Barlow lens is carried in the eye-piece, having an effect equivalent to doubling the focal length of the mirror. The spectroscope used has a collimating lens of $\frac{3}{4}$ inch diameter and 5 inches focal length, and the prisms are so arranged that the rays first traverse the lower part and then are reflected back through the upper part of them to the observing telescope. In my early measures a number equivalent to 4 of 60° were used, and a power of 15 on the observing telescope, but I have lately used 5 and 6 prisms (1 or 2 half-prisms and 2 whole ones of 60° twice over) and a power of 25.

I have tried many methods of comparison. I will enumerate the chief of them. First, the comparison light from the hydrogen vacuum tube was, by reflection, mixed with the rays from the star on the slit of the spectroscope; secondly, the comparison light was reflected on the slit by the side of the star, so that the spectra were viewed side by side. Both these methods rendered the detection of any noncoincidence easy; but the dark line of the spectrum of the star was often put out in the first method, and in the second the measurement of the displacement was difficult and uncertain when made with a bright-wire-micrometer both fixed in the eye-piece, and with the wires reflected by the surface of the last prism, so that the images only were seen. This was probably owing to the lines whose distances apart were to be measured not being side by side, and also being slightly curved.

I have therefore adopted the following method. The vacuum tube is carried by two wooden clips, which also embrace at their other extremities a glass rod fixed to the spectroscope in such a

position that the tube can be swung at pleasure in front of the slit or below the level of the same; the spectrum of the star and that of the tube can then be viewed alternately by a slight motion of the latter. The displacement of the line is measured as follows:—A brass tube 8 inches long carries a collimating lens at one end, and in its focus at the other is a piece of tinfoil, with a minute hole in it, moved by a micrometer screw; the tube is so fixed that the spot of light from the tinfoil is reflected from one of the surfaces of the last prism, or the last but one, and is focussed in the eye-piece with the spectrum viewed. In the case of the first prism of the train being a half one with its surface last passed by the rays normal to the axis of the observing telescope, the last surface of the next one must be used for the purpose of reflecting the micrometer spot; and since it has then to pass through one half-prism to the observing telescope, it would be drawn out into a spectrum, if it were not illuminated by a monochromatic or nearly monochromatic light. The hole in the tinfoil is therefore illuminated by a small swing spirit lamp, with a coil of platinum wire holding common salt over the wick. The spectra of the star and tube are alternately thrown into the field, and the spot of light made to coincide with each in its turn, and the readings of the screw are noted; by subtraction the value of the displacement in terms of the divisions of the screw is then obtained. The value of the latter in wave-lengths is obtained by measuring the distance between the F-line and any other line of known position near it in the solar spectra, or by measurement of the distance between the magnesium lines produced by the electric spark. In my apparatus about 4 divisions of the screw correspond to an alteration of one tenth-metre in wave-length. The vacuum tube is placed with its length at right angles to the slit, the latter of which is placed so that the star would traverse its length by its apparent diurnal motion. A cylindrical lens is used in front of the slit, for I find a considerable width of spectrum advantageous, and conducive to accuracy. The foregoing arrangements work well under existing conditions here, but they are no doubt capable of improvement.

I would remark that a less dispersive power, or its equivalent, than that I have been using, seems to me to be almost valueless for anything like reliable results, and in fact a greater instrumental power is the thing now to be obtained; and since the image of a star does not increase in size by increase of aperture and corresponding focal length of telescope, assuming perfection of workmanship we may make the image of the star on the slit intrinsically brighter almost without optical limit, while leaving the angle of the cone of rays constant; an increase of aperture will therefore render the spectrum brighter, or enable a greater dispersive power to be used with undiminished brightness of spectrum.

The subjoined list of measures explains itself sufficiently, except perhaps as to the sixth column, headed "Corrected motion

of Star," which contains the observed velocity of the star in miles per second, corrected for the value of the Earth's orbital motion, the sign + meaning that the star is receding from, and − approaching, our system.

Spectroscopic Observations of the Motion of Stars in the line of Sight made at the Temple Observatory, Rugby.

No.	Name of star.	No. of prisms.	Line compared.	No. of measures.	Corrected motion of star. Miles per sec. +	Date 1800 +	Remarks.
1	γ Pegasi	5	F	2	+ 9·7	78·85	
2	Rigel	4	F	4	+29·3	77·15	Line badly defined.
3	γ Orionis	4	F	4	+41·58	77·05	
4	,,	5	F	4	+18·1	79·16	
5	δ Orionis	5	F	3	+54·3	79·16	
6	ε Orionis	5	F	3	+13·3	79·16	
7	ζ Orionis	5	F	3	+16·5	79·16	
8	α Orionis	4	b_4	3	−96·9	77·15	Moonlight; doubtful.
9	,,	4	F	2	+25·4	77·05	
10	,,	4	F	est.	+24·9	77·09	
11	,,	5	b_4	5	+32·9	79·17	
12	β Aurigæ	5	F	3	+ 0·6	79·17	Star line nebulous.
13	Sirius	4	F	2	+84·4	77·05	
14	Castor	6	F	2	+71·1	78·25	Star line hazy.
15	,,	5	F	3	+14·8	79·17	
16	,,	5	F	3	+23·7	79·25	
17	Pollux	4	F	2	+57·7	77·20	Very doubtful and faint.
18	,,	5	F	1	−27·7	79·17	Very doubtful.
19	,,	5	F	2	+26·6	79·25	
20	Procyon	4	F	5	−43·6	77·15	Moonlight.
21	,,	4	F	4	+ 2·2	77·20	
22	,,	4	F	2	+70·2	78·11	
23	,,	4	F	3	+22·8	78·24	
24	,,	6	F	2	+53·2	78·25	
25	,,	5	F	3	−45·1	79·16	
26	,,	5	F	4	+12·7	79·17	
27	,,	5	F	3	+43·1	79·25	
28	η Leonis	6	F	3	+21·9	78·31	
29	Regulus	6	F	3	+57·3	78·25	
30	,,	6	F	4	− 2·7	78·31	
31	,,	6	F	5	+67·8	78·33	
32	,,	5	F	5	+36·1	79·17	

No.	Name of star.	No. of prisms.	Line compared.	No. of measures.	Corrected motion of star. Miles per sec.	Date 1800 +	Remarks.
33	„	5	F	3	+16·6	79·25	Line nebulous.
34	„	5	F	2	+27·0	79·33	
35	„	7	F	3	+35·1	79·33	
36	ε Ursæ Major.	5	F	3	−32·0	79·33	
37	δ Leonis	6	F	3	−59·5	78·33	
38	„	5	F	3	+ 8·18	79·33	Line nebulous.
39	θ Leonis	6	F	4	−67·4	78·33	Very doubtful and faint.
40	„	5	F	2	+44·6	79·33	Very faint and doubtful.
41	β Leonis	6	F	3	−73·5	78·25	Line very nebulous.
42	„	6	F	5	+32·2	78·31	„ „
43	„	6	F	3	+20·9	78·33	
44	„	5	F	3	−41·8	79·33	
45	η Ursæ Major.	6	F	4	+ 7·8	78·46	
46	Arcturus	4	b_1	2	+53·7	77·45	
47	„	6	b_1	1	−33·0	78·31	
48	„	7	F	3	− 7·2	79·33	
49	ζ Boötis	6	F	2	−27·3	78·47	Doubtful and very faint.
50	α Cor. Bor.	6	F	3	+ 6·5	78·46	
51	„	6	F	3	+22·3	78·47	
52	α Ophiuchi	6	F	3	−15·4	78·46	
53	Vega	4	F	4	−94·0	77·57	
54	„	6	F	3	−14·5	78·47	Doubtful.
55	„	6	F	6	−16·8	78·69	
56	„	6	F	4	−41·0	78·71	
57	Altair	6	F	3	−44·0	78·69	
58	„	6	F	4	+15·2	78·71	
59	„	6	F	3	+26·0	78·72	
60	δ Cygni	6	F	4	−29·4	78·71	
61	„	6	F	3	−57·8	78·72	
62	γ Cygni	6	F	3	−49·2	78·71	
63	„	6	F	4	−43·0	78·78	Doubtful.
64	ε Cygni	6	F	2	+20·2	78·78	
65	α Cygni	6	F	3	−39·1	78·69	
66	„	6	F	3	−56·9	78·71	
67	„	6	F	4	+45·5	787·8	Very doubtful.
68	„	5	F	4	−36·0	78·85	

Preliminary Paper on the Babylonian Astronomy.

By R. H. M. Bosanquet, Esq., and Professor A. H. Sayce.

The Calendar.

The progress of the decipherment of the Inscriptions of Assyria and Babylon has rendered it possible to develop with some certainty the fundamental principles of the Astronomy of those ancient nations. We are at present engaged in the study of the Babylonian Astronomy; and we desire to state that we have obtained certain results, and to give some indication of their nature.

The first question is, What was the nature of the Babylonian Calendar, and how did it depend on the motions of the heavenly bodies? Of this question we have been able to obtain a solution of an apparently complete nature. So far as we are aware, this calendar has never been discussed before; and it is as remarkable for its theoretical elegance as for its practical simplicity and historic interest. We do not say that every passage in the inscriptions harmonises with this calendar. Many of the inscriptions are very obscure; many are hopelessly corrupt copies of earlier ones; and many contain accounts of phenomena which are absolutely impossible on any hypothesis whatever. Under these circumstances the discussion of the evidence of the inscriptions is a matter requiring care and caution. But there is in our minds no doubt worth consideration that the calendar in question was substantially the calendar of those who wrote the inscriptions.

In the present paper we propose to quote a few of the principal passages on which the establishment of the calendar rests, and to give some account of it. Other subjects, such as the identification of stars, the measurement of longitude, and the treatment of the evidence generally, must await another opportunity.

First, we must premise that the inscriptions are written in at least two languages, Assyrian and Accadian; and everything has in consequence two names at least, though these are generally represented by the same character. The planets also frequently assume the names of stars in whose neighbourhood they were observed. For the present we will only take the case of the star *Icu* or *Dilgan*, which appears to have been the foundation of the calendar in question. It was called *Dilgan*, or 'the messenger of light,' in Accadian, and "Icu of Babylon" in Assyrian. The object of mentioning this is to explain how it is that the two names are used indifferently.

The following are the principal passages connecting this star with the beginning of the year.

In the reverse of the Catalogue, or Preface, of the *Illumi-*

nation of Bel (the great ancient astronomical work of Babylon), there occurs this passage, among the general enumeration of the phenomena and the astronomer's duties :—

"The appearance at the beginning of the year of the star *Icu*, ... one observes."

On the colophon of a tablet containing an enumeration of the stars of certain months there occurs, by way of contents, what is supposed to be the first line of the next tablet; it runs :—

"The star *Icu* in the month Nisan was seen."

The language of these two inscriptions is Semitic. They belong, however, to an ancient work compiled for Sargon of Agané about 1700 B.C.

These two passages prepare us to find that the definition of time by a star consists in its appearing; whereas it has commonly been supposed that the definition of time by a star consists in its disappearing behind the Sun.

The only kind of annual appearing that stars perform, for the purposes of those who watch all night, is their heliacal rising, or issuing forth from the region of the Sun before sunrise The frequent mention of the rising of stars confirms us in the opinion that the heliacal rising was the phenomenon by which they were chiefly classified. We were therefore prepared by these passages to find that the beginning of the year was regulated by the heliacal rising of some star.

The following passage now requires consideration : "When on the first day of the month Nisan the star of stars (or *Dilgan*) and the Moon are parallel, that year is normal. When on the third day of the month Nisan the star of stars and the Moon are parallel, that year is full" (i.e. has 13 months).

This inscription is in Accadian, the most ancient language of the inscriptions. It may be expected to belong to a time earlier than 2000 B.C.

On considering this inscription from the point of view of modern astronomy, it becomes evident that we have here the key to the calendar, if we can only ascertain definitely that the months were lunar months; so that the beginning of the year could be defined by the Moon. As there is some difficulty about this point, it is necessary to consider it a little.

The following passage occurs in the Preface or Catalogue before mentioned :—

"12 months to each year, ($6 \times 60 =$) 360 days, in order are recorded by the hand . . . ."

If this was accurate, the months could not be lunar. But it is abundantly clear that they were lunar. The more recent inscriptions are the clearest on this point. They record cases of watching for the Moon at the end of the month.

The notices of eclipses throughout the inscriptions serve to confirm this conclusion. The eclipses of the Moon were generally observed about the 14th day, and those of the Sun from the

28th to the 30th. In some of the ancient astrological tablets there are collocations of observations of eclipses which present difficulties; but these still recur month after month on the same days, so that the lunar determination of the month is preserved throughout.

Further, the names of these months are substantially those of the lunar calendar of the Jews. It is therefore to be expected that the determination of the beginnings of the months will be found to be effected according to the custom of that calendar, which is by the observation of the first visible appearance of the new Moon.

(For a description of the Jewish custom *see* Disraeli's *Alroy*, Part x. ch. 1, "We are the watchers of the Moon, to tell the nation that the month begins.")

There is only one way of reconciling the calendar of lunar months with the statement as to the year of 360 days. The mean synodic lunation being about $29\frac{1}{2}^{d}$, the real length would often be 30^{d}. We must suppose that the reckoning up of the 12 sets of 30 days to the total of 360^{d} was the work of a commentator, or perhaps of an inaccurate writer or copyist. We have, in other parts of these tablets, examples of statements undoubtedly incorrect, which are to be ascribed to similar causes.

In order, however, to ascertain the possibility of working a lunar calendar according to the rule of 360 days, we have discussed the calendar thus resulting. And we find that undoubtedly a calendar of 360 days in the year can be worked approximately by lunar methods; but the rule on which the working of such a calendar would depend would be entirely different from that given in the inscriptions; while, the months being of 30 days, and not lunar months, the watchings for the new Moon and the eclipses would occur irregularly on all the different days of the month. And this would be contrary to what we find in the inscriptions.*

Assuming then that the months were lunar, and began with the first visible appearance of the new Moon, we proceed to deduce the nature of the calendar arising from the rule above quoted, viz.:—

"When on 1 Nisan the star of stars and the Moon are parallel, that year is normal. When on 3 Nisan the star of stars and the Moon are parallel, that year is full," i.e. has 13 months. We will first consider this rule independently of anything else,

* We may just mention that the lunar rule of the calendar of 360^{d} would depend on the fact that this period exceeds the lunar year of 12 lunations by the same amount, roughly, by which it falls short of the solar year; so that the lunar intercalary months would accumulate approximately at the same rate as those required to make up the solar year.

In fact

$$360^{d} = 12 \text{ mean lunations} + 5{\cdot}633^{d}$$
$$= 1 \text{ tropical year} - 5{\cdot}242^{d}$$

and afterwards take up its relation to the equinox, and the heliacal rising of the same star (*Dilgan* or *Icu*) at the beginning of the year.

We will consider this relation, "when the Moon is parallel to the star of stars," as defining a point on the ecliptic, which we will regard as the origin of longitude. And we will speak of longitude thus measured as "longitude from *Icu*." It will be positive when measured in the usual direction, from W. to E., and negative in the opposite direction.

Assume further, for small distances from *Icu*, that the Moon's mean distance from the Sun is 12° for each day from new Moon; that the Sun's daily motion is 1°; and the Moon's daily motion 13°.

The new Moon is said to be visible under the most favourable circumstances 18 hours after conjunction. The probable cases would therefore be included between the limits 18 and 42 hours after conjunction. We assume 30 hours for the mean time of visibility after conjunction.

In the mean normal year of the rule the Moon is, then, parallel to *Icu* on 1 Nisan, 30 hours after conjunction, or at a distance of 15° from the Sun. Hence,

Sun's longitude from *Icu* on 1 Nisan in the normal year is $-15°$.

In the "full year" of the rule the Moon is parallel to *Icu* on 3 Nisan, or in the mean $3\frac{1}{4}^{d}$ from the Sun. Hence,

Sun's longitude from *Icu* on 3 Nisan mean full year $= -39°$;

and, according to the rule, when the Sun is as much as this behind *Icu* on 3 Nisan an intercalary month is due at the end of the year.

The most remarkable thing about the rule so far is the way in which the Moon is used as a mere pointer in the sky, for measuring the distance from the Sun to a fixed point among the stars. For the comprehension of the actual working of this rule in each separate case, as above, nothing is needed but the very roughest knowledge of the Moon's mean motion for a few days. Without going any further, we see that the rule secures on the average an accurate sidereal year. For whenever the Sun, at the beginning of the year, has slipped more than a certain distance from a fixed point among the stars, an intercalary month is added to bring him up again.

We will now form the table of the succession of the years, and the introduction of the intercalary months, according to the approximate mean rule of lunations used in modern calendars.

According to this approximation, $1^{y} - 11^{d} = 12$ lunations. We can enumerate all the possible cases conveniently by supposing that the years succeed each other in the order thus determined, without return after the 19 years period. We can then always find some part of the table which shall approximately represent a given case.

As the lunar years are too short, let the Moon of 1 Nisan come x days too soon in the general case. Then x is a number

similar to the epact of modern calendars, and the Sun will be $x°$ behind his place in the normal year. Then,

			1 Nisan. °	3 Nisan. °
Normal Year	Longitude from *Icu*	Moon	0	26
,,	,, ,,	Sun	-15	-13
General Year	,, ,,	Sun	$-(15+x)$	$-(13+x)$
,,	,, ,,	Moon	$-x$	$26-x$

since the mean distances of the Sun and Moon on 1 and 3 Nisan are 15° and 39° respectively.

Table of the cases of the Babylonian Calendar, arranged in the order shown by a mean calendar Moon, without return after 19 *years period.*

	Sidereal years. y		$-x$ Days over. d		Intercalary month.		Lunar years.	At the beginning of following year. Moon's longitude from *Icu.* 3 Nisan $=26°-x°$. °	Intercalary Due at 7°.
Normal Year	1	—	11			=	1	15	
	2	—	22			=	2	4	I. D.
	3	—	33	+	30	=	3	23	
	4	—	14			=	4	12	
	5	—	25			=	5	1	I. D.
	6	—	36	+	30	=	6	20	
	7	—	17			=	7	9	
	8	—	28			=	8	−2	I. D.
	9	—	39	+	30	=	9	17	
	10	—	20			=	10	6	I. D.
	11	—	31	+	30	=	11	25	
	12	—	12			=	12	14	
	13	—	23			=	13	3	I. D.
	14	—	34	+	30	=	14	22	
	15	—	15			=	15	11	
	16	—	26			=	16	0	I. D.
	17	—	37	+	30	=	17	19	
	18	—	18			=	18	8	
	19	—	29			=	19	−3	I. D.
	20	—	40	+	30	=	20	16	
	21	—	21			=	21	5	I. D.
	22	—	32	+	30	=	22	24	
	23	—	13			=	23	13	
	24	—	24			=	24	2	I. D.
	25	—	35	+	30	=	25	21	
	26	—	16			=	26	10	
	27	—	27			=	27	−1	I. D.
	28	—	38	+	30	=	28	18	
	29	—	19			=	29	7	I. D.
	30	—	30	+	30	=	30	26	

The rule for the intercalary month says, "when the Moon is parallel to *Icu* on the 3rd Nisan." But since the Moon moves through about 13° each day, we must admit half a day's journey, say 6° or 7°, to come within the rule.

And we must take 7° at least. For if we do not, the condition of the normal year can never recur; i.e. the parallelism to *Icu* on the 1st Nisan, or $x = 0$. We see from years 29 and 30 of the table that this happens when the longitude from *Icu* on 3 Nisan is 7° the year before, and the intercalary is then due. Of course it is not probable that the rule was worked to a single degree in this way; but for calculation we must draw the line somewhere, and we select 7° as the limit for the above reasons. It is remarkable that this rule of intercalaries leads to a series of values for x which are in all respects comparable with the epacts of the ecclesiastical calendars.

It will be seen that the values of x vary from 0 in the normal year to 29 in the 19th year of the series. That is to say, the Sun is always later than in the normal year, and may be as much as 29 days later. This is obviously a property of any calendar regulated by intercalary months; every true annual occurrence must be liable to an apparent oscillation of date amounting to the inside of a month.

So far we have said nothing about equinoxes; we have established the calendar, in accordance with the rule of the inscriptions, on a purely sidereal basis. We can now proceed to consider the probable position, with respect to the equinox, of our starting place—the point marked on the ecliptic as parallel to the star of stars.

There is reason to believe that the mean date of the equinox was not far from the beginning of Nisan. If we assume for the mean year No. 15 of the table, in which the Sun is 15° behind the normal position, or midway between its extreme positions, we have for the Sun's longitude from *Icu* on 1 Nisan, −30°. At the date of the establishment of the calendar, we may therefore expect to find the equinox about 30° behind the star *Icu*, or the star *Icu* about 30° in front of the equinox, with some considerable latitude either way. (For we cannot be sure that the mean position of the equinox would coincide with 1 Nisan, within 15 days or more either way.)

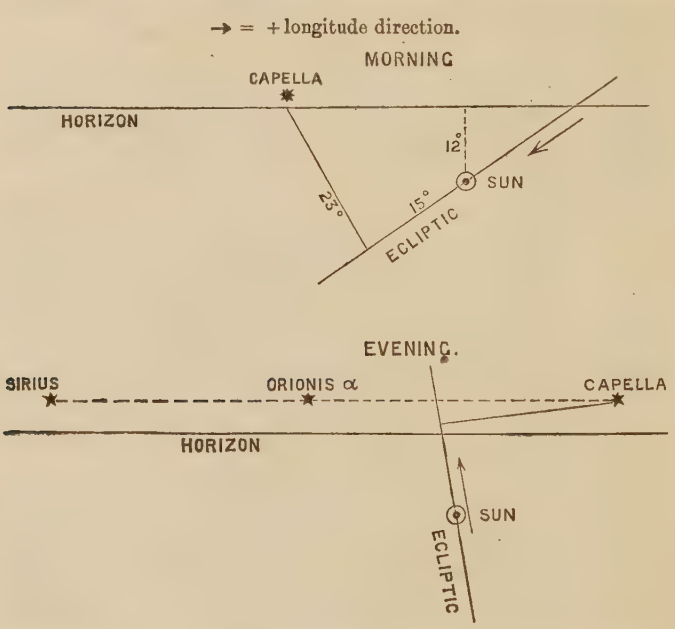
It is generally admitted that the establishment of this calendar is to be looked for about 2000 B.C. We select the number of 3,960 years, equivalent to 55° of precession, and seek for a star which was in longitude 30°±15° about 3,960 years ago; we seek especially for any star of the first magnitude which also satisfies the condition of rising heliacally about the time of the new year. There is no doubt about the result. The star *Capella* is the only star that satisfies these conditions; and numerous independent lines of evidence converge to identify it with *Icu*. It rose heliacally at the period in question before it set; its large north latitude, 22° 51′ 45″ (Delambre), or 23° nearly, throwing it up in the

morning, at the vernal equinox, when the ecliptic has its least inclination to the horizon.

The identification was effected by means of a celestial globe, on which the positions of the principal stars have been laid down for the period in question, referred to the equator and ecliptic of the globe as the equator and ecliptic of the period. But the following calculation exhibits the result as to the longitude:—

	°	′	″
Longitude of *Capella* with regard to *Regulus* (Delambre)	292	0	40
Longitude of *Regulus*, 1840, Airy (Smyth's Cycle)	147	36	20
	439	37	0
	360	0	0
	79	37	0
Precession	55	0	0
Longitude of *Capella* 2120 B.C.	24	37	0

The latitude of Babylon may be taken at $32\frac{1}{2}°$. We have generally solved the problem of the heliacal rising approximately



by means of the globe. And considering the vagueness of our information about the phenomenon itself, we think perhaps that this method is near enough for the purpose. Assuming that a 1^m star rises heliacally when it is 12° in direct altitude above the

Sun at the horizon, we find that *Capella* rose heliacally when the Sun was about 15° short of the star in longitude. But this is the position of the Sun on 1 Nisan in the normal year, subject to a small correction. So that the coincidence of the beginning of the year with the rising of *Icu* is fairly satisfied by the position of *Capella* at the given date.

To explain the occurrence of these different phenomena at morning and evening, at the same time of the year, we subjoin the following figures, which represent roughly the appearance of the quarters of the sky in the neighbourhood of the rising and setting Sun respectively at this time.

The evening position, "parallel to *Capella*," for a body moving in the ecliptic, is marked by a nearly level line of stars at about the same altitude, the other chief stars being *α Orionis* and *Sirius*. This line cuts the ecliptic some 3° above the foot of the latitude perpendicular from *Capella*.

The intersection of this horizontal line with the ecliptic is possibly to be regarded as the true starting-point of the calendar. The allowance, to be added to the equinoctial longitude of *Capella* on this account, varies a little with the change of position of the starry sphere; but it is not far from 3° within somewhat wide limits, both of precession and diurnal motion.

Then the longitude of the point "parallel to *Icu*" would be *Capella's* longitude + 3° = 27½° say. Whence

1 Nisan	Sun's longitude in normal year	=	$12\frac{1}{3}^{\circ}$ { from Equinox 2120 B.C. }
	„ „ mean year	=	$-2\frac{1}{2}$
	„ „ latest year	=	$-16\frac{1}{2}$

and

		d
Earliest equinox	=	$-12\frac{1}{2}$ Nisan
	(=	$17\frac{1}{2}$ Adar)
Mean „	=	$2\frac{1}{2}$ Nisan
Latest „	=	$16\frac{1}{2}$ „

The equinox will vary with the date, being earlier for later dates and later for earlier dates, to the extent of 1ᵈ for every 71 years nearly.

In the present paper we have explained the most essential parts of our conclusions as to the Babylonian Calendar. We hope, on a future occasion, to discuss other portions of the Babylonian Astronomy.

An Invention for giving perfectly uniform Rotary Motion in Driving Clocks. By H. C. Russell, Esq., Director of the Sydney Observatory.

The Clock now to be described was designed to give uniform motion to a Barrel-Chronograph, and the result is perfectly satisfactory; so much so that the driving weight may be varied 30 per cent. without producing *any change* in the rate.

I have applied the same form of governor to a telescope clock, and it keeps a star on the wire as if it formed part of it; and there is good reason to suppose that the same governor applied to the train of a standard clock would give better results than any yet obtained from the single pendulum.

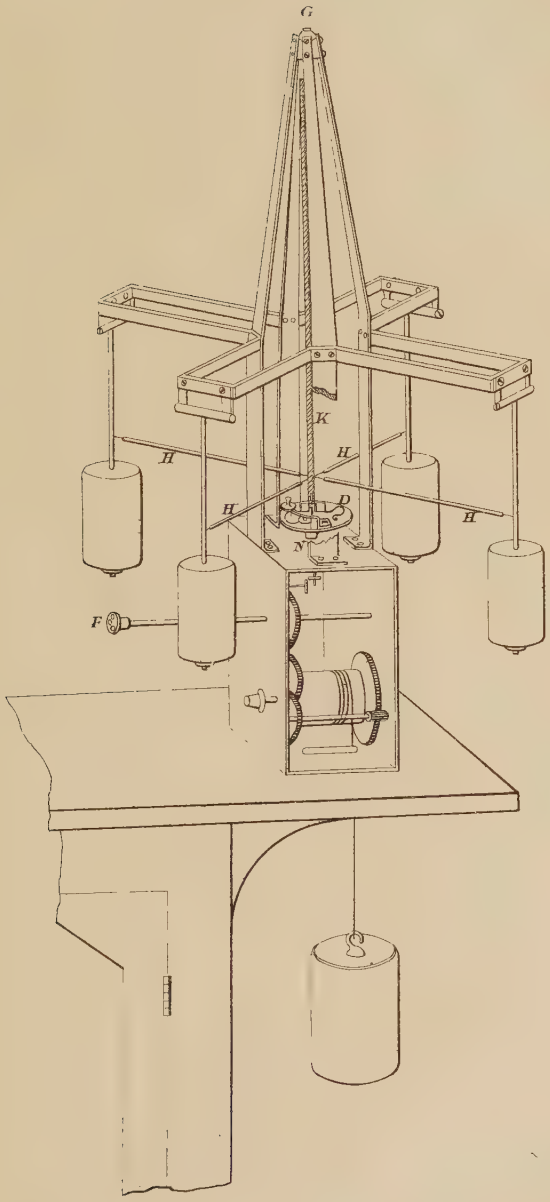
These are strong statements; but they will, I think, be confirmed by what follows:—

In design the clock is very simple, and it seems strange that a similar arrangement was not long since devised. It will be easily understood by the aid of the following short description and two diagrams.

Fig. I. is a general view of the clock, showing the train of wheels, ending in a pair of bevel wheels at the top of the clock case. The object of these is to make the escape wheel axis vertical, and to have it project through the top of the clock case at N enough to carry the wheel D (Figs. I. and II.) and the parts attached to it, which are (1) the arm A (Fig. II.) projecting from the first wheel in the train; (2) the spring B; (3) a small train of wheels C, ending in a fan-fly which makes two hundred revolutions for one of the first wheel J; (4) projecting up from the arm A at the point E is a small steel pin half an inch long, which, when at rest, forms a continuation of the axis of the wheel D, but when in motion leaves the centre, taking the arm A and thus turning the wheel J and setting the fan in motion. This pin E forms the connection between the clock train and the pendulums, for it works in a hole in the lower end of the rod K, and this is connected with each of the pendulums by the rods H, H, H, H; they are fastened to K by small steel springs, and to the pendulum rods by small ball and socket joints. The rod K, together with part of the weight of the rods H, H, H, H, is held up by a steel wire at G.

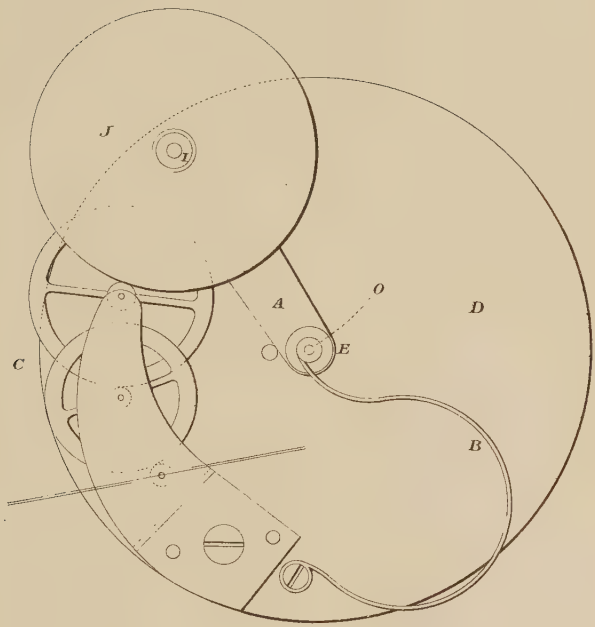
The axis of the wheel J and the arm A is a steel pin fixed to, and standing up vertical from, the surface of the wheel D; the arm A therefore moves in a plane parallel to the surface of D, and when it leaves the position shown in the drawing it carries the pin E along the dotted curve EO, E being always vertical. In moving from the centre, E bends the light steel spring B, which acts as a resisting force to the increase of its eccentricity. It is obvious that if E moves slowly the wheels and fan will offer very little, if any, resistance; but it cannot be moved suddenly, owing to the resistance offered by the train and fan. The

Fig. 1.



WCODE & Co. LITH. LONDON

Fig. 2.



WOODE & Co LITH. LONDON.

strength of the spring B must be learned by experiment; if it is too strong, the clock will gain with increased weight, and if it is too weak the clock will lose under the same conditions. The one in use in this clock is about as strong as the balance spring in an American clock.

There is nothing unusual about the pendulums, except that they are hung by two springs instead of one; the bobs are lead, and each of them weighs 15 lb.; the time is half a second. The train is an ordinary clock train, and requires no mention here.

When all the parts of this clock are in the position shown, and a weight is applied to the driving barrel, the wheel D begins to move, and a slight impulse given to one of the pendulums pushes the pin E from the centre of D, and it at once sets the other three pendulums in motion, and their arcs gradually increase until the force necessary to drive them is equal to the driving weight. Should the weight be increased, the pendulum arcs, and with them the eccentricity of E, increase until equilibrium is again attained. Now, if the experiment of increasing the weight be tried without the spring B, the clock will be found to go slower, like an ordinary pendulum clock under increased weight; but if the spring B be carefully adjusted, 30 per cent. added to the weight will not produce *any change in rate*, even when the test is a first-class sidereal clock marking the chronograph cylinder every second, a test which, as is well known, would at once show the smallest change of rate.

One difficulty, which in the earlier experiments promised to be a very serious one, was overcome by introducing the small train of wheels. It was this: if all the pendulums were not perfectly isochrone, the pin E would very soon cease to move in a circle, and would describe an ellipse, and have therefore an *irregular* rotary motion. Independent of the difficulty of making four pendulums isochrone, it is necessary that the escapement should have in it something to prevent the pin E from moving in an ellipse, and such the train of wheels and fan provide, because they strongly resist sudden changes of eccentricity, at the same time that they allow slow changes, such as those caused by friction, to go on with little or no resistance. In this is involved an important principle, viz.: If the pendulums are not isochrone, the time given in each revolution of the escape wheel is the *mean* of the times of a double oscillation for each pendulum. So that, if four nearly compensated pendulums in a standard clock were thus connected, the rate under changes of temperature would be the mean of the four rates. And since the spring, as before shown, counteracts the effect of changes in the friction or driving power, there is good reason to suppose that such a clock would keep better time than a clock with only one pendulum, subject to the many changes of impulse given to it by the train.

Such is the clock now used to work the barrel chronograph from which the sample sheets sent herewith have been taken.

Some of these have been run without change of weight, in one the weight has been changed twice, in another only one change; but I have confined the experiments so far to a change of 30 per cent. in the driving weight, because they take so much time to make them.

It is necessary to add that this clock is the first made of the kind, and the wheels are old ones adapted to the requirements; but in some of them imperfections are known to exist, and these appear as slight errors in the seconds marks. I have been obliged to send out of the colony to get the wheels made with the accuracy required, and it will be some time before the clock intended for the chronograph is made; but the results with this rough experimental clock are so good that I thought they ought to be published.

Four pendulums are not essential; two give very good results provided the arms H be not too short, say not less than 12 inches. Such a clock I have applied to the 11½-inch Equatoreal, and the motion is perfect, keeping a star on the wire as if it were part of it, so long as changing refraction does not interfere. By a very simple contrivance the rate is changed to suit the Moon or planets. This is effected by continuing the pendulum rods two inches below the bobs and fixing brass disks on the end of them; upon these weights can be placed having a known effect upon the rate, and for convenience they are stamped with the number of seconds change they produce in ten minutes; so that it is only necessary to look at the *Nautical Almanac* and see the Moon's change in R.A. in ten minutes, and select these weights, in order to make the telescope follow the Moon exactly.

A few words about the origin of the clock may not be out of place here. Last year a new barrel chronograph was required for Sydney Observatory, and Mr. William Barraclough, of Sydney, undertook to make it. When the cylinder and pen carriage were completed, experiments were made with several well-known forms of governor, in order to secure uniform rotation of the cylinder. None of these would give such uniform motion as I required; and Mr. Barraclough said that some years since, when in England, he had been asked to make a clock that would not *tick*, and that after some trouble he had succeeded, using only one pendulum; but that he thought he could make perfectly uniform motion with two oscillating pendulums, and at my request he made a rough model with two pendulums working at right angles to each other. Each pendulum was connected with the eccentric pin by means of a double (Watt's) parallel motion, so arranged that the part of the rod connected with E moved in a straight line parallel to the surface of the wheel D; and the motion of the eccentric pin was made stiff by means of a cloth washer under the arm A; this obliged E to move in a circle, and at the same time allowed the pendulum arcs to increase if necessary. This model proved that it was possible to get smooth rotary motion from two oscillating pen-

dulums; but when it came to be tested for uniform rotation, I found the friction of the cloth washer so uncertain that the governor was of little value. Several other forms of friction under the arm A gave a like result: without any friction under A, elliptical motion was always sooner or later set up by the pin E, and at one time I almost gave up hope of overcoming the difficulty; for these experiments made it obvious that E must be *free* to move and yet have some *resistance to sudden change of position*, so that it could not move in an ellipse. I then devised the small train and fan-fly to meet these requirements, and the four straight rods connecting the pendulums with the rod K, instead of the rather complex parallel motion of the model. In its present form the clock seems only to require for its perfection that the spring B shall not be changed by change of temperature, and this I have no doubt can be accomplished by making it of a particular length and form.

I have purposely avoided dimensions, because these may be varied at pleasure. For instance, seconds pendulums may be used and the wheel D would then turn in two seconds; but I prefer and have used half-seconds pendulums. So the weight of the pendulums and the strength of their supporting springs may vary; but it is not desirable to have the supporting springs stronger than is necessary to carry the weight. The only dimension that perhaps ought to be given is the length of the rods H. If these are short they do not work well. Those I have in use are nine inches long, and they would work better if longer, because they would then move near the direction of the swing of the pendulum and not tend to twist it so much, as they move from side to side with the eccentric.

Sydney Observatory,
1879, *March* 18.

On the Applicability of the Mean Refractions of Bessel's "Fundamenta" to the Washington Observations. By A. M. W. Downing, Esq.

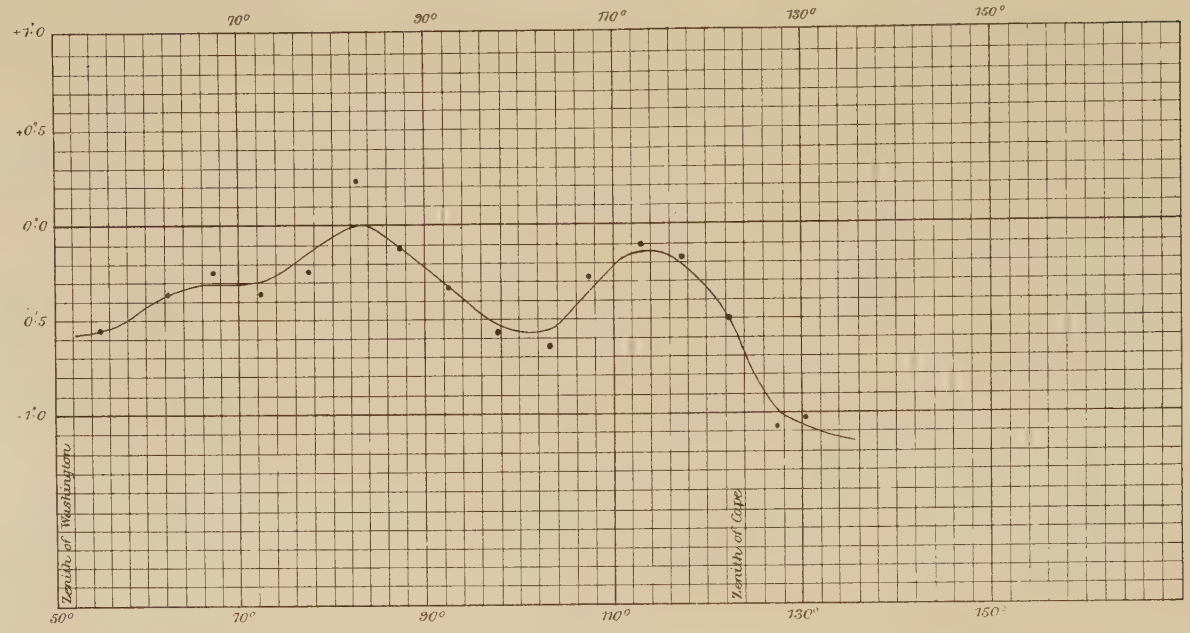
As it appears from recent investigations, the results of which are exhibited in an Addendum to the Introduction to the Greenwich Nine-Year Catalogue, that the refractions of the *Tabulæ Regiomontanæ* unaltered, at all events as far as 85° Z.D., represent fairly enough the Greenwich observations, I thought that it would be interesting to try whether the N.P.D.'s of the Washington Catalogue for 1860 are in satisfactory accord with observations made in the Southern Hemisphere, as these N.P.D.'s are reduced with the refractions of the *Tabulæ* unaltered. I have

accordingly made a comparison between the N.P.D.'s of the Cape (1860) and Washington Catalogues—a work of no small magnitude, as there are just 400 stars common to the Catalogues which are available for the comparison, omitting those whose places depend on a single observation. The stars have been arranged in order of N.P.D., and the proper motions given in the Cape Catalogue applied to the Washington places; they have then been taken in groups of 5° each, and the means of the differences taken for each group. These mean differences have been laid down on cross-ruled paper, and a curve (*see* diagram) drawn through the points. The following table gives the differences as computed and as read off from the curve :—

Z.D. at Washington.	N.P.D.	Number of Stars.	Cape—Washington.	
			Means.	Curve.
° °	° ′		″	″
0—9	55 24	8	−0·53	−0·53
9—14	62 32	19	−0·37	−0·37
14—19	67 45	38	−0·25	−0·30
19—24	72 13	16	−0·35	−0·30
24—29	77 33	24	−0·23	−0·16
29—34	82 31	27	+0·23	0·00
34—39	87 30	23	−0·11	−0·11
39—44	92 35	23	−0·32	−0·32
44—49	97 55	27	−0·57	−0·53
49—54	103 14	14	−0·63	−0·56
54—59	107 20	33	−0·28	−0·36
59—64	113 4	22	−0·10	−0·16
64—69	117 26	59	−0·18	−0·22
69—74	122 10	40	−0·50	−0·50
74—78	127 2	14	−1·09	−0·97
78—82	130 23	8	−1·03	−1·07
82—85	134 10	2	−3·50	—
85—87	136 49	2	−3·85	—

In order to assure myself of the reality of the discordances in the last three groups, I have computed the differences of the Washington and Melbourne (1870) places south of N.P.D. 129°, the Melbourne places being reduced to the Cape standard by the table given on page 176 of the present volume of the *Monthly Notices*. The following is the result :—

COMPARISON OF THE NORTH POLAR DISTANCES OF THE CAPE (1860) AND WASHINGTON (1860) CATALOGUES.



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N.P.D. ° ′	Number of Stars.	Melbourne—Washington. ″
130 16	16	−1·32
134 25	3	−3·62
136 49	2	−3·96

The difference at Z.D. 82°—85° is untrustworthy, as one of the stars employed, occurring in both the Cape and Melbourne comparisons, is very discordant; if this star, whose place in the Washington Catalogue depends on only two observations, be rejected, the mean of the comparisons with the places in the Southern Catalogues is −1″·55. I propose to adopt this as the difference for N.P.D. 134° 21′.

The readings from the curve for every 5° (with computed values below 80° Z.D.) then give—

Z.D. ° ′	N.P.D. ° ′	Cape—Washington. ″
4	55	−0·55
9	60	−0·43
14	65	−0·33
19	70	−0·31
24	75	−0·25
29	80	−0·07
34	85	−0·05
39	90	−0·22
44	95	−0·44
49	100	−0·56
54	105	−0·50
59	110	−0·23
64	115	−0·17
69	120	−0·36
74	125	−0·80
79	130	−1·07
83 15	134 21	−1·55
85 43	136 49	−3·85

It is evident that, in addition to discordances arising from other causes than refraction, the tabular refractions used at Washington must be diminished in order that the observations may agree with those made at the Cape. Now, as far as 85° Z.D. the mean refractions of the *Fundamenta* are those of the *Tabulæ* diminished in the proportion of 1 : 0·996718, and a preliminary calculation showed that this was very nearly the proportion in which the Washington refractions should be diminished. By substituting the mean refractions of the *Fundamenta* for those of the *Tabulæ*, the discordances are reduced as follows:—

Z.D.				Z.D.			
°	″		″	°	″		″
4	−0·55	to	−0·54	49	−0·56	to	−0·34
9	−0·43		−0·40	54	−0·50		−0·24
14	−0·33		−0·28	59	−0·23		+0·07
19	−0·31		−0·24	64	−0·17		+0·23
24	−0·25		−0·17	69	−0·36		+0·13
29	−0·07		+0·03	74	−0·80		−0·13
34	−0·05		+0·07	79	−1·07		−0·15
39	−0·22		−0·07	83¼	−1·55		+0·01
44	−0·44		−0·26	85¾	−3·85		−0·62

If then the Washington observations were reduced with the mean refractions of the *Fundamenta* and the meteorological corrections of the *Tabulæ*, the agreement with the Cape observations would be very satisfactory if we may assume that the relative correction for errors of the adopted latitudes is small.

It appears from this investigation, and also from the comparison of the Cape and Greenwich Catalogues referred to above, that the tabular refractions used in the reduction of the Cape observations also require to be somewhat diminished.

1879, *June* 10.

Ephemeris for Physical Observations of Mars, 1879–80. By A. Marth, Esq.

Greenwich Noon.	Angle of Posit. of ♂'s Axis.	Areographical W. Long. of the Centre of ♂'s Disk.	Diff.	Lat.	Dia-meter.	Amount of Greatest Defect of Illumination.	Posit.	Areocentric Ang. between Earth & Sun.
1879.	°	°	°	°	″	″	°	°
July 29	143·34	299·53		−17·64	10·38	1·66	250·39	47·11
			700·63					
31	143·19	280·16		17·29	10·51	1·67	250·68	47·03
			·64					
Aug. 2	143·06	260·80		−16·93	10·64	1·69	250·98	46·93
			·66					
4	142·95	241·46		16·58	10·78	1·70	251·28	46·82
			·68					
6	142·85	222·14		16·22	10·92	1·72	251·58	46·70
			·71					
8	142·77	202·85		15·87	11·06	1·73	251·89	46·56
			·73					
10	142·71	183·58		15·52	11·21	1·74	252·20	46·40
			·75					
12	142·66	164·33		15·16	11·36	1·75	252·52	46·22
			·78					
14	142·62	145·11		14·81	11·51	1·76	252·84	46·03
			·79					
16	142·60	125·90		14·47	11·67	1·77	253·15	45·81
			·82					
18	142·59	106·72		14·12	11·83	1·77	253·47	45·58
			·85					

Greenwich Noon.	Angle of Posit. of ♂'s Axis.	Areographical W. Long. of the Centre of ♂'s Disk.	Diff.	Lat.	Diameter.	Amount of Greatest Defect of Illumination.	Posit.	Areocentric Ang. between Earth & Sun.
1879.	°	°	°	°	″	″	°	°
Aug. 20	142·60	87·57		13·79	11·99	1·78	253·79	45·32
			·88					
22	142·61	68·45		13·46	12·16	1·78	254·11	45·04
			·90					
24	142·64	49·35		13·13	12·34	1·79	254·42	44·74
			·93					
26	142·67	30·28		12·81	12·52	1·79	254·74	44·41
			·95					
28	142·71	11·23		12·50	12·70	1·78	255·05	44·06
			700·99					
30	142·76	352·22		12·20	12·89	1·78	255·35	43·68
			701·02					
Sept. 1	142·82	333·24		−11·91	13·08	1·78	255·65	43·27
			·04					
3	142·88	314·28		11·63	13·27	1·77	255·94	42·84
			·08					
5	142·94	295·36		11·37	13·48	1·76	256·23	42·37
			·11					
7	143·01	276·47		11·11	13·68	1·75	256·51	41·88
			·14					
9	143·08	257·61		10·87	13·89	1·73	256·78	41·35
			·18					
11	143·15	238·79		10·64	14·11	1·71	257·04	40·78
			·22					
13	143·22	220·01		10·43	14·33	1·69	257·29	40·18
			·25					
15	143·29	201·26		10·24	14·55	1·66	257·53	39·54
			·30					
17	143·36	182·56		10·07	14·78	1·63	257·75	38·85
			·34					
19	143·42	163·90		9·91	15·02	1·60	257·96	38·13
			·38					
21	143·48	145·28		9·78	15·25	1·56	258·16	37·36
			·42					
23	143·53	126·70		9·67	15·49	1·52	258·34	36·55
			·47					
25	143·58	108·17		9·58	15·73	1·47	258·50	35·69
			·51					
27	143·62	89·68		9·51	15·98	1·42	258·65	34·79
			·56					
29	143·65	71·24		9·46	16·22	1·37	258·77	33·83
			701·61					
Oct. 1	143·67	52·85		−9·44	16·47	1·31	258·88	32·83
			·66					
3	143·68	34·51		9·45	16·72	1·25	258·97	31·77
			·71					
5	143·68	16·22		9·48	16·96	1·19	259·03	30·66
			·76					
7	143·68	357·98		9·54	17·20	1·12	259·08	29·49
			·82					
9	143·66	339·80		9·62	17·44	1·04	259·10	28·26
			·87					
11	143·63	321·6		9·73	17·67	0·96	259·09	26·98
			·92					

Greenwich Noon.	Angle of Posit. of ♂'s Axis.	Areographical W. Long. of the Centre of Diff.	Lat. ♂'s Disk.	Dia-meter.	Amount of Greatest Defect of Illumination.	Posit.	Areocentric Ang. between Earth & Sun.
1879.	°	°	°	″	″	°	°
Oct. 13	143·59	303·59	9·88	17·90	0·88	259·06	25·65
		701·98					
15	143·54	285·57	10·05	18·12	0·80	259·01	24·25
		702·03					
17	143·48	267·60	10·24	18·32	0·72	258·93	22·80
		·08					
19	143·41	249·68	10·46	18·51	0·63	258·82	21·30
		·13					
21	143·33	231·81	10·71	18·69	0·55	258·70	19·74
		·18					
23	143·25	213·99	10·98	18·85	0·47	258·54	18·13
		·22					
25	143·16	196·21	11·27	18·99	0·39	258·36	16·48
		·27					
27	143·07	178·48	11·58	19·11	0·32	258·15	14·79
		·31					
29	142·97	160·79	11·91	19·20	0·25	257·92	13·05
		·34					
31	142·88	143·13	12·26	19·27	0·19	257·65	11·29
		702·36					
Nov. 2	142·78	125·49	−12·62	19·32	0·13	257·34	9·49
		·40					
4	142·68	107·89	12·98	19·34	0·09	257·0	7·68
		·41					
6	142·59	90·30	13·36	19·32	0·05	256·5	5·84
		·43					
8	142·50	72·73	13·74	19·28	0·03	255·8	4·00
		·43					
10	142·42	55·16	14·12	19·22	0·01	254·4	2·15
		·43					
12	142·35	37·59	14·49	19·12	0·00	...	0·06
		·43					
14	142·28	20·02	14·86	18·99	0·00	...	1·52
		·42					
16	142·22	2·44	15·22	18·84	0·02	77·9	3·26
		·39					
18	142·16	344·83	−15·57	18·66	0·04	77·1	5·12
		·38					
20	142·11	327·21	15·91	18·46	0·07	76·6	6·88
		·34					
22	142·07	309·55	16·23	18·23	0·10	76·16	8·60
		·30					
24	142·04	291·85	16·53	17·99	0·15	75·83	10·27
		·27					
26	142·01	274·12	16·81	17·73	0·19	75·56	11·91
		·22					
28	141·99	256·34	17·17	17·46	0·24	75·31	13·49
		·18					
30	141·97	238·52	17·30	17·17	0·29	75·08	15·01
		702·13					
Dec. 2	141·95	220·65	−17·52	16·87	0·35	74·88	16·49
		·08					
4	141·94	202·73	17·71	16·57	0·40	74·69	17·91
		702·03					

Greenwich Noon.	Angle of Posit. of ♂'s Axis.	Areographical W. Long. of the Centre of ♂'s Disk.	Diff.	Lat.	Diameter.	Amount of Posit. of Greatest Defect of Illumination.		Areocentric Ang. between Earth & Sun.
1879.	°	°	°	°	″	″	°	°
Dec. 6	141·92	184·76		17·88	16·25	0·46	74·53	19·28
			701·98					
8	141·91	166·74		18·02	15·94	0·51	74·38	20·58
			·92					
10	141·90	148·66		18·14	15·62	0·56	74·25	21·83
			·87					
12	141·89	130·53		18·24	15·30	0·61	74·14	23·02
			·82					
14	141·88	112·35		18·32	14·98	0·66	74·05	24·15
			·76					
16	141·88	94·11		18·38	14·66	0·70	73·97	25·22
			·71					
18	141·87	75·82		18·41	14·35	0·74	73·90	26·23
			·66					
20	141·86	57·48		18·42	14·04	0·78	73·85	27·19
			·61					
22	141·85	39·09		18·41	13·74	0·81	73·82	28·10
			·56					
24	141·84	20·65		18·38	13·44	0·84	73·80	28·95
			·52					
26	141·83	2·17		18·33	13·14	0·87	73·80	29·75
			·47					
28	141·82	343·64		18·26	12·85	0·89	73·81	30·50
			·43					
30	141·82	325·07		18·17	12·57	0·91	73·83	31·21
			701·39					
1880.								
Jan. 1	141·81	306·46		−18·07	12·30	0·93	73·87	31·87
			·35					
3	141·80	287·81		17·95	12·03	0·94	73·92	32·49
			·32					
5	141·80	269·13		17·81	11·77	0·95	73·98	33·07
			·28					
7	141·81	250·41		17·66	11·52	0·96	74·06	33·61
			·25					
9	141·82	231·66		17·49	11·28	0·97	74·15	34·11
			·21					
11	141·83	212·87		17·30	11·04	0·97	74·25	34·57
			·19					
13	141·85	194·06		17·10	10·81	0·98	74·37	34·99
			·15					
15	141·88	175·21		16·89	10·58	0·98	74·49	35·39
			·13					
17	141·91	156·34		16·66	10·37	0·98	74·63	35·75
			·11					
19	141·95	137·45		16·42	10·16	0·97	74·79	36·07
			·08					
21	141·99	118·53		16·17	9·95	0·97	74·95	36·37
			·06					
23	142·05	99·59		15·91	9·75	0·96	75·12	36·65
			·04					
25	142·11	80·63		15·63	9·56	0·96	75·31	36·89
			·02					
27	142·19	61·65		15·34	9·38	0·95	75·51	37·11
			701·00					

Greenwich Noon.	Angle of Posit. of ♂'s Axis.	Areographical W. Long. of the Centre of ♂'s Disk.	Diff.	Lat.	Diameter.	Amount of Posit. of Greatest Defect of Illumination.		Areocentric Ang. between Earth & Sun.
1880.	°	°	°	°	″	″	°	°
Jan. 29	142·27	42·65		15·04	9·20	0·94	75·72	37·31
			700·98					
31	142·36	23·63		14·73	9·03	0·93	75·94	37·48
			700·97					
Feb. 2	142·47	4·60		−14·41	8·86	0·92	76·16	37·64
			·95					
4	142·58	345·55		14·08	8·70	0·91	76·40	37·77
			·94					
6	142·71	326·49		13·74	8·54	0·90	76·66	37·88
			·92					
8	142·85	307·41		13·39	8·39	0·89	76·92	37·97
			·92					
10	143·00	288·33		13·03	8·24	0·88	77·19	38·04
			·90					
12	143·16	269·23		12·66	8·10	0·86	77·47	38·09
			·89					
14	143·34	250·12		12·28	7·96	0·85	77·77	38·13
			·88					
16	143·53	231·00		11·89	7·83	0·84	78·07	38·15
			·86					
18	143·73	211·86		11·50	7·70	0·82	78·38	38·16
			·86					
20	143·94	192·72		11·10	7·57	0·81	78·70	38·15
			700·86					
22	144·17	173·58		−10·69	7·45	0·79	79·03	38·13

1879 Aug. 14. Winter solstice of *Mars'* northern hemisphere.
1880 Jan. 21. Spring equinox „ „ „

The observations of the position-angles of the south solar spot, made in 1877, by Asaph Hall (*Astron. Nach.*, No. 2174), and by Schiaparelli (*Osservazioni astronomiche e fisiche sull' asse di rotazione e sulla topografia del planeta Marte.* Roma, 1878), indicate that the determination of the direction of the planet's axis, deduced from the scanty observations of 1830–37, requires considerable corrections. A trustworthy new determination will only be feasible, in case proper observations are secured during at least the two next oppositions, 1879 and 1881–82. For this purpose several methods of observing will have to be tried and tested, in order that the best method for giving correct results may be ascertained. The want of sufficient agreement between the position-angles of Hall and Schiaparelli, which is obviously due to their different modes of observing, and which must be cleared up before the observations can be used with confidence, leaves considerable doubt respecting the amount of the correction of the predicted position-angles of the axis in 1877; but since both series indicate that the assumed inclination is chiefly in fault, I have thought it right to introduce in the present Ephemeris preliminary corrections, which will probably diminish the differences between the predicted and

observed position-angles of the axis, and which will considerably reduce the amount of the ultimate corrections required. The values, adopted in the computations, of the inclination and node of the plane of the equator of *Mars* in reference to that of the Earth are for 1880·0, inclination 36°·260, node 47°·945. As in previous cases, the data of the Ephemeris are to be interpolated directly for the times of the observations, the equation of light having already been duly taken into account. The amount q, and the position-angle Q of the greatest defect of illumination, may serve in reducing position-angles and distances, which have been observed in reference to the assumed centre of the illuminated disk, to the true centre of the planet. But the reductions depend on what is assumed by observers as the centre of the illuminated disk. If that point is fixed upon which bisects that diameter of the disk, which is perpendicular upon the line of cusp, or in position-angle Q, the distance between the true centre and the assumed centre is $\frac{1}{2}q$. But if the centre of gravity of the apparent disk or the point, lines laid through which bisect the illuminated area, is assumed to be the observed centre, its distance from the true centre is $\frac{4q}{3\pi}$. If p_1 and s_1 are the observed angle and distance referred to the assumed centre, the values p and s referred to the true centre are found in the first case by

$$s \sin (p-p_1) = \tfrac{1}{2}q \sin (p_1-Q),$$
$$s \cos (p-p_1) = s_1-\tfrac{1}{2}q \cos (p_1-Q),$$

or, when s_1 is not small, by approximate formulæ. In the second case, $\frac{4q}{3\pi} = [9{\cdot}6278]$, q is to be substituted for $\frac{1}{2}q$.

A reference must be sufficient to the remarks made on page 307 of vol. xxxvii. respecting observations of the times and places of the passages across the central meridian of all the most distinct and well-defined points on the planet's surface which may serve as fundamental points of Areography. If observers cannot be induced to make these observations, the topography of the surface of *Mars* must remain in an unsatisfactory state.

The following list gives the areographic longitude and latitude of the centre of *Mars*, and also its apparent diameter, for the times of a number of sketches made chiefly during the oppositions of 1871, 1873, and 1877. A similar list of the sketches of 1862 and 1864 is to be found in the *Monthly Notices*, vol. xxxvii., pp. 305–307, and another in the *Astronomical Register*, vol. xv., pp. 153–154.

The present list comprises the sketches of—

Boeddicker, O. (5 plates with 10 sketches in the *Veröffentlichungen von der Kgl. Sternwarte zu Göttingen*, 1878. It is assumed that m. Z. B. means "mittlere Zeit Berlin.")

Burton, Ch. (6 sketches, published in M. T. Terby's *Aréographie*, Bruxelles, 1874. Figs. 10, 11, 20, 30, 42, 47.)

Crossley & Gledhill.	(1 sketch in Terby's *Aréographie*. Fig. 18.)
Cruls, Luiz.	(13 plates with 26 sketches in *Observatoire Impérial de Rio de Janeiro*, "Mémoire sur *Mars*." In the 6 cases marked corr., the assigned times are the corrected ones, not those on the plates.)
Dreyer, J.	(12 sketches, with "Notes on the physical appearance of the planet *Mars*, as seen with the Three-Foot Reflector at Parsonstown, during the opposition of 1877," in the *Scientific Transactions of the Royal Dublin Society for the year* 1878.)
Gledhill, J.	(4 sketches in Terby's *Aréographie*. Figs. 41, 46, 51, 52.)
Green, N.	(6 sketches of 1873 in the *Astronomical Register* for July, 1873. 5 of these sketches are reproduced in Terby's *Ar.* (Figs. 15, 43, 44, 48, 49.)
	(12 sketches of 1877, made at Madeira, to be published in Vol. xliv. of the *Memoirs of the Royal Astronomical Society*.)
Knobel, E. B.	(17 sketches of 1873 in the *Monthly Notices*, vol. xxxiii., p. 476, and an additional sketch in Terby's *Ar.* Fig. 31.)
Knott, G.	(5 sketches in Terby's *Ar.* Figs. 9, 26, 33, 34, 40.)
Lehardelay	(2 sketches in Terby's *Ar.* Figs. 32, 54, the times being assumed to be Paris times.)
Lohse, O.	(6 sketches of 1871, Nos. 5, 7, 8, 10, 11, 12 on Tafel 6 of the *Astron. Beobachtungen zu Bothkamp*.)
	(12 sketches of 1877 on Tafel 8 of the *Publicationen des Astrophysikalischen Observatoriums zu Potsdam*. No. 2.)
	(8 sketches of 1873, represented by woodcuts on pp. 127, 128 of the same publication.)
Niesten, L.	(5 plates with 42 sketches in "*Observations sur l'Aspect physique de la planète Mars pendant l'opposition de* 1877. Brux. 1877.)
Schmidt, J.	(5 sketches in Terby's *Ar.* Figs. 8, 16, 17, 21, 39.)
Secchi, A.	(1 sketch of 1864 in Terby's *Ar.* Fig. 45.)
Terby, M. F.	(12 sketches, fig. 15–26, in "Observations de *Jupiter* et de *Mars* faites à Louvain . . . en 1873." *Bulletin de l'Académie R. de Belgique*, vol. 36, No. 11.)
	(15 sketches in "Etudes sur la planète *Mars*," 11me notice. *Bulletin*, vol. 45, No. 1. The times are assumed to be Brussels times.)
Trouvelot, L.	(4 sketches on plate 22 of vol. 8 of the *Annals of Harvard College Observatory*.)
Vogel, H.	(3 sketches, Nos. 4, 6, 9, on Tafel 6 of the *Astron. Beobachtungen zu Bothkamp*.)
Webb, T. W.	(3 sketches in Terby's *Ar.* Figs. 13, 29, 53.)
Weinek, L.	(3 sketches in the paper *Sirius*, vol. xii., 1.)
Wilson, T. M.	(3 sketches in Terby's *Ar.* Figs. 14, 22, 50.)

Where the times of the commencement and of the completion of the sketch are given, the longitude of the centre of the disk refers to the mean of the times.

Areographical Long. and Lat. of the Centre of the Disk.		Diameter.		Times assigned to the Sketches.				
°	°	″				h m	h m	
2·4	−22·4	24·8	Cruls VII. 1	1877	Sept. 1	7 30		Rio de Janeiro.
5·2	+21·1	16·2	Green, No. 5 (*Ar.* f. 48)	73	May 16	9 15		Greenwich.
6·6	−22·4	24·8	Green 1	77	Sept. 1	10 40		Greenwich.
6·8	+25·1	14·3	Lehardelay (*Ar.* f. 32)	71	Mar. 23	10	—11	Paris.
7·5	+22·1	15·6	Trouvelot 2	73	May 24	9 30		Cambridge, Mass.
11·1	−24·2	20·4	Niesten III. 1	77	Oct. 6	8		Brussels.
14·8	−24·4	20·0	Niesten III. 3	77	Oct. 8	9 30		Brussels.
16·6	−24·5	19·8	Niesten III. 5	77	Oct. 9	10 30		Brussels.
18·7	+20·5	16·4	Knobel 7	73	May 12	7 45		Greenwich.
19·0	+25·0	14·3	Webb (*Ar.* f. 29)	71	Mar. 22	10 35		Greenwich.
19·4	−22·4	24·7	Terby 1	77	Aug. 30	10 30	—10 45	Brussels.
19·7	−24·2	20·4	Terby 13	77	Oct. 6	8 30	— 8 40	Brussels.
21·5	−24·4	20·0	Dreyer 11	77	Oct. 8	9 40		Greenwich.
24·2	+20·6	16·4	Terby f. 23	73	May 12	8 20	— 8 30	Brussels.
25·7	−24·1	20·8	Terby 12	77	Oct. 4	7 40	— 7 50	Brussels.
26·2	+21·5	16·0	Knobel 12	73	May 19	12 30		Greenwich.
27·8	−27·1	14·0	Niesten V. 1	77	Nov. 10	6 55		Brussels.
29·4	−24·1	20·8	Niesten II. 8	77	Oct. 4	8 (in text 11)		Brussels.
30·1	−22·5	24·8	Cruls VII. 2	77	Sept. 2	10 0		Rio de Janeiro.
30·9	+21·1	16·2	Knobel 10	73	May 16	11 0		Greenwich.

Areographical Long. and Lat. of the Centre of the Disk.		Diameter.		Times assigned to the Sketches.				
°	°	″				h m	h m	
35·9	+25·1	14·3	Gledhill (*Ar.* f. 46)	1871	Mar. 23	12 20		Greenwich.
40·1	+25·0	14·3	Lohse No. 7	71	Mar. 22	12 42		Bothkamp.
41·8	+20·3	16·5	Terby f. 20	73	May 10	8 10—	8 40	Brussels.
43·2	−22·4	24·8	Green 2	77	Sept. 1	13 10		Greenwich.
43·8	+22·9	13·0	Knobel (*Ar.* f. 31)	73	June 20	9		Greenwich.
43·9	+25·1	14·3	Burton (*Ar.* f. 30)	71	Mar. 23	12 25—	12 30	Dublin.
45·3	+22·0	15·7	Trouvelot 1	73	May 23	11 30		Cambridge, Mass.
47·7	−24·2	20·4	Niesten III. 2	77	Oct. 6	10 30		Brussels.
47·8	+20·6	16·4	Lohse f. 4 and 5	73	May 12	10 25		Bothkamp.
?49·0	−24·0	21·0	Dreyer 10	77	Oct. 3	?11 10	(about)	Greenwich.
49·5	+20·4	16·4	Knobel 5	73	May 11	9 15		Greenwich.
50·3	−23·7	21·8	Terby 10	77	Sept. 29	6 20		Brussels.
50·8	−22·4	24·7	Cruls V. 2	77	Aug. 29	9 0		Rio de Janeiro.
53·1	−24·0	21·0	Lohse 9	77	Oct. 3	9 36		Berlin.
57·9	+22·9	13·1	Lohse f. 8	73	June 19	10 0		Bothkamp.
60·3	−22·4	24·8	Green 3	77	Sept. 1	14 20		Greenwich.
60·4	+20·4	16·4	Knobel 6	73	May 11	10 0		Greenwich.
64·1	−24·1	20·6	Niesten II. 9	77	Oct. 5	11		Brussels.
64·7	+20·7	16·3	Green No. 6 (*Ar.* f. 49)	73	May 13	11 30		Greenwich.
65·4	−22·4	24·7	Cruls VI. 1	77	Aug. 29	10 0		Rio de Janeiro.

65·5	+24·8	14·2	Vogel No. 6	71	Mar. 18	12 4	Bothkamp.
66·0	−24·7	20·7	Knott (*Ar.* f. 33)	62	Oct. 22	8 30	Greenwich.
66·2	+20·3	16·5	Terby f. 21	73	May 10	10 5	Brussels.
67·4	−23·7	21·8	Niesten II. 7	77	Sept. 29	7 30	Brussels.
68·1	−23·1	21·6	Terby 11	77	Sept. 30	8 5— 8 15	Brussels.
70·6	+20·1	16·5	Lohse f. 3	73	May 9	10 10	Bothkamp.
71·0	+20·3	16·5	Terby f. 22	73	May 10	10 25	Brussels.
76·9	+20·7	16·3	Knobel 9	73	May 13	12 20	Greenwich.
78·1	−26·5	14·2	Knott (*Ar.* f. 34)	62	Nov. 27	7 15	Greenwich.
80·4	−22·4	24·4	Cruls III. 1	77	Aug. 24	8 0	Rio de Janeiro.
81·0	−22·4	24·2	Niesten I. 2	77	Aug. 22	10	Brussels.
83·3	+20·6	16·4	Knobel 8	73	May 12	12 10	Greenwich.
84·7	+20·1	16·5	Burton (*Ar.* f. 47)	73	May 9	9 24—10 40	Dublin.
			(Long. at commencement of sketch 75°·4, at completion 93°·9)				
85·4	−23·5	22·2	Niesten II. 5	77	Sept. 27	7 30	Brussels.
86·1	−23·6	22·0	Terby 9	77	Sept. 28	8 5— 8 15	Brussels.
87·4	−22·4	24·7	Cruls VI. 2	77	Aug. 29	11 30	Rio de Janeiro.
89·9	−22·4	24·1	Niesten I. 1	77	Aug. 21	10	Brussels.
89·9	+19·9	16·5	Lohse f. 2	73	May 8	9 53	Bothkamp.
92·0	−26·6	15·1	Niesten V. 2	77	Nov. 3	6 45	Brussels.
93·6	−23·7	21·8	Green 4	77	Sept. 29	9	Greenwich.
95·0	−22·4	24·4	Cruls III. 2	77	Aug. 24	9 0	Rio de Janeiro.

Areographical Long. of the Centre of the Disk. °	Areographical Lat. of the Centre of the Disk. °	Diameter. "		Times assigned to the Sketches.		h m	h m	
96·4	−23·5	22·2	Terby 7	1877	Sept. 27	8 15		Brussels.
97·8	+19·9	16·5	Wilson (*Ar.* f. 50)	73	May 8	10 45		Greenwich.
100·5	−26·6	15·1	Niesten V. 3	77	Nov. 3	7 20		Brussels.
101·1	−23·8	21·4	Dreyer 9	77	Oct. 1	10 45 (in text $10^h\ 55^m$)		Greenwich.
103·3	−25·5	24·8	Cruls VIII. 1	77	Sept. 2	15 0 *corr.*		Rio de Janeiro.
104·3	−23·5	22·2	Terby 8	77	Sept. 27	8 40	8 55	Brussels.
109·6	−22·4	24·4	Cruls IV. 1	77	Aug. 24	10 0		Rio de Janeiro.
111·1	+24·4	13·9	Lehardelay (*Ar.* f. 54)	71	Mar. 11	10	11	Paris.
112·4	−26·6	15·3	Niesten IV. 12	77	Nov. 2	7 30		Brussels.
114·1	−23·5	22·3	Lohse 8	77	Sept. 26	9 27		Berlin.
114·6	−23·5	22·2	Niesten II. 6	77	Sept. 27	9 30		Brussels.
116·5	−23·4	22·5	Lohse 7	77	Sept. 25	9 0		Berlin.
130·2	−23·2	23·0	Niesten II. 2	77	Sept. 22	7 30		Brussels.
130·8	−22·4	24·1	Green 5	77	Aug. 21	12 30		Greenwich.
132·7	−23·7	21·8	Weinek 3	77	Sept. 29	12·5		Leipzig.
135·5	−23·6	21·9	Dreyer 8	77	Sept. 28	11 15		Greenwich.
135·8	−23·2	23·0	Lohse 6	77	Sept. 22	8 29		Berlin.
136·3	−23·4	22·5	Niesten II. 4	77	Sept. 25	9 45		Brussels.
140·4	−22·4	24·4	Cruls II. 2	77	Aug. 23	11 30		Rio de Janeiro.
141·2	−23·2	23·0	Niesten II. 3	77	Sept. 22	8 15		Brussels.

143·9	−23·5	22·1	Cruls XII. 2	77	Sept. 27	8 20	Rio de Janeiro.
147·3	+22·9	14·2	Knobel 17	73	June 8	8 30	Greenwich.
148·4	− 6·6	17·3	Secchi (*Ar.* f. 45)	64	Dec. 1	7	Rome.
150·1	−23·2	23·2	Niesten I. 7	77	Sept. 21	8 15	Brussels.
151·9	−23·2	23·2	Terby 6	77	Sept. 21	8 15— 8 30	Brussels.
155·3	−23·5	22·3	Cruls XII. 1	77	Sept. 26	8 30	Rio de Janeiro.
160·3	−23·2	23·2	Lohse 5	77	Sept. 21	9 33	Berlin.
161·1	−23·2	23·2	Niesten I. 8	77	Sept. 21	9	Brussels.
163·9	+18·4	16·5	Terby f. 19	73	Apr. 29	10 5 —10 15	Brussels.
165·0	−22·4	23·9	Cruls I. 2	77	Aug. 19	10 45	Rio de Janeiro.
166·2	−22·5	23·6	Cruls I. 1	77	Aug. 16	9 0	Rio de Janeiro.
167·5	−26·0	16·5	Niesten IV. 9	77	Oct. 26	6 45	Brussels.
170·5	−23·0	23·5	Lohse 4	77	Sept. 20	9 38	Berlin.
174·1	−23·2	23·0	Niesten I. 9	77	Sept. 22	10 30	Brussels.
175·5	−23·1	23·3	Green 6	77	Sept. 20	9 5	Greenwich.
185·8	−26·0	16·5	Niesten IV. 10	77	Oct. 26	8	Brussels.
189·3	−22·4	24·0	Cruls II. 1	77	Aug. 19	15 30 *corr.*	Rio de Janeiro.
190·0	−22·9	23·7	Terby 4	77	Sept. 17	8 30— 8 35	Brussels.
191·8	+24·0	13·3	Lohse No. 5	71	Mar. 2	11 9	Bothkamp.
193·5	−22·9	23·7	Lohse 3	77	Sept. 17	9 23	Berlin.
194·0	−23·2	23·2	Niesten II. 1	77	Sept. 21	11 15	Brussels.
201·1	−22·4	24·4	Cruls IV. 2	77	Aug. 24	16 15 *corr.*	Rio de Janeiro.

Areographical Long. and Lat. of the Centre of the Disk.		Diameter.		Times assigned to the Sketches.				
°	°	"				h m h m	
201·4	+17·7	16·3	Terby f. 18	1873	Apr. 25	10 15—10 25	Brussels.
207·7	−26·0	16·5	Niesten IV. 11	77	Oct. 26	9 30	Brussels.
209·1	−23·0	23·6	Green 7	77	Sept. 18	10 20	Greenwich.
210·4	+22·7	14·8	Lohse f. 7	73	June 2	9 45	Bothkamp.
211·5	+17·9	16·4	Knobel 4	73	Apr. 26	11 20	Greenwich.
213·0	+22·5	15·1	Green No. 1	73	May 30	7 30	Greenwich.
213·4	−22·2	23·0	Schmidt (*Ar.* f. 39)	62	Oct. 1	7 28	Athens.
215·0	−22·9	23·7	Terby 5	77	Sept. 17	10 15	Brussels.
215·7	−22·4	24·4	Cruls V. 1	77	Aug. 24	17 15 *corr.*	Rio de Janeiro.
218·6	−22·9	23·7	Niesten I. 6	77	Sept. 17	10 30	Brussels.
220·1	+17·3	16·2	Terby f. 17	73	Apr. 23	10 10—10 40	Brussels.
220·2	−22·9	23·9	Cruls XI. 1	77	Sept. 16	6 50	Rio de Janeiro.
220·5	−23·2	23·1	Weinek 2	77	Sept. 21	13·6	Leipzig.
221·1	+24·0	13·2	Vogel No. 4	71	Feb. 28	11 57	Bothkamp.
221·7	−22·8	24·0	Niesten I. 5	77	Sept. 15	9 30	Brussels.
226·6	−23·0	22·6	Knott (*Ar.* f. 40)	62	Oct. 8	11	Greenwich.
226·8	−23·6	22·1	Cruls XIII. 1	77	Sept. 27	14 0 *corr.*	Rio de Janeiro.
227·4	−25·4	17·6	Niesten IV. 7	77	Oct. 20	7	Brussels.
229·0	−22·9	23·7	Dreyer 7	77	Sept. 17	10 55	Greenwich.
232·2	−25·4	17·6	Niesten IV. 6	77	Oct. 20	7 20	Brussels.

232·3	−23·0	23·6	Green 8	77	Sept. 18	11 45	Greenwich.
232·4	+25·9	13·8	Lohse No. 10	71	Apr. 8	11 23	Bothkamp.
234·5	+22·5	15·2	Terby f. 26	73	May 29	8 30–– 8 35	Brussels.
234·7	+22·6	15·0	Knobel 16	73	May 31	9 30	Greenwich.
235·4	+17·3	16·2	Knobel 2	73	Apr. 23	11 40	Greenwich.
236·7	−26·0	16·5	Niesten IV. 8	77	Oct. 26	11 30	Brussels.
236·8	+25·9	13·7	Lohse No. 11	71	Apr. 9	12 17	Bothkamp.
237·1	−25·4	17·6	Niesten IV. 5	77	Oct. 20	7 40	Brussels.
237·2	+26·0	13·6	Lohse No. 12	71	Apr. 10	12 55	Bothkamp.
237·9	−22·8	24·1	Niesten I. 4	77	Sept. 14	10	Brussels.
237·9	−22·9	23·5	Dreyer 6	77	Sept. 16	10 55	Greenwich.
239·9	+22·4	15·3	Green No. 2 (*Ar.* f. 43)	73	May 28	8	Greenwich.
240·9	−22·8	24·1	Terby 3	77	Sept. 14	10 —10 25	Brussels.
241·0	−22·9	23·8	Cruls XI. 2	77	Sept. 16	8 15	Rio de Janeiro.
244·0	+25·8	13·7	Gledhill (*Ar.* f. 52)	71	Apr. 8	11 30	Greenwich.
247·0	+25·8	13·9	Gledhill (*Ar.* f. 41)	71	Apr. 6	10 30	Greenwich.
248·2	−22·7	24·3	Boeddicker IV. 2	77	Sept. 12	10 5·9	Berlin.
248·7	+22·6	15·1	Knobel 15	73	May 30	9 50	Greenwich.
250·4	−22·8	24·0	Green 9	77	Sept. 15	11 10	Greenwich.
250·5	−25·4	17·6	Niesten IV. 4	77	Oct. 20	8 35	Brussels.
251·2	+17·3	16·2	Knobel 3	73	Apr. 23	12 15	Greenwich.
253·3	−25·3	18·0	Niesten IV. 3	77	Oct. 18	7 30	Brussels.

Areographical Long.	Areographical Lat. of the Centre of the Disk.	Diameter.		Times assigned to the Sketches.			
°	°	″				h m h m	
253·5	+22·5	15·2	Burton (*Ar.* f. 20)	1873	May 29	9 — 9 15	Dublin.
256·5	−22·8	24·0	Dreyer 5	77	Sept. 15	11 35	Greenwich.
256·7	+16·8	16·0	Lohse f. 1.	73	Apr. 20	11 30	Bothkamp.
257·0	+22·0	15·7	Schmidt (*Ar.* f. 21)	73	May 23	7 41	Athens.
259·8	+27·4	10·9	Wilson (*Ar.* f. 22)	71	May 12	9	Greenwich.
261·6	+16·5	15·8	Terby f. 16	73	Apr. 18	10 5—10 25	Brussels.
?265·3	−22·7	24·3	Boeddicker V. 1	77	Sept. 12	*circa* 11 16	Berlin.
265·9	+25·9	13·8	Burton (*Ar.* f. 42)	71	Apr. 7	11 39—12 18	Dublin.
266·5	−22·6	24·5	Green 10	77	Sept. 10	9 15	Greenwich.
266·9	−22·6	24·5	Lohse 2	77	Sept. 10	10 10	Berlin.
267·8	−22·7	24·3	Cruls IX. 2	77	Sept. 12	7 40	Rio de Janeiro.
268·7	−22·7	24·4	Terby 2	77	Sept. 11	10 5—10 30	Brussels.
269·0	+16·8	16·0	Knobel 1	73	Apr. 20	11 40	Greenwich.
271·7	−22·7	24·4	Niesten I. 3	77	Sept. 11	10 30	Brussels.
271·8	+25·7	14·0	Crossley & Gledhill (*Ar.* f. 18)	71	Apr. 4	11	Greenwich.
272·6	−24·7	24·3	Boeddicker V. 2	77	Sept. 12	11 45·6	Berlin.
274·8	−22·6	24·6	Lohse 1	77	Sept. 8	9 30	Berlin.
275·5	+25·7	14·0	Webb (*Ar.* f. 53)	71	Apr. 4	11 15	Greenwich.
279·4	−22·7	24·3	Dreyer 4	77	Sept. 12	11 20	Greenwich.
280·5	+21·9	15·8	Schmidt (*Ar.* f. 16)	73	May 22	8 41	Athens.

281·1	−25·0	18·6	Niesten III. 8	77	Oct. 15	7 30	Brussels.
282·2	+25·8	13·9	Burton (*Ar.* f. 10)	71	Apr. 6	12 6—12 52	Dublin.
283·1	+22·0	15·6	Terby f. 25	73	May 24	8 40— 8 55	Brussels.
283·7	+22·4	15·3	Green No. 3 (*Ar.* f. 44)	73	May 28	11	Greenwich.
283·7	−22·7	24·3	Cruls X. 1	77	Sept. 12	8 45	Rio de Janeiro.
283·7	−21·6	23·0	Schmidt (*Ar.* f. 8)	62	Sept. 26	8 36	Athens.
284·5	−25·0	18·6	Lohse 12	77	Oct. 15	8 20	Berlin.
284·6	−25·4	17·6	Niesten IV. 2	77	Oct. 20	10 55	Brussels.
286·7	−22·6	24·5	Cruls IX. 1	77	Sept. 10	7 45	Rio de Janeiro.
290·7	+22·2	15·5	Lohse f. 6	73	May 25	10 5	Bothkamp.
291·4	+27·2	11·3	Gledhill (*Ar.* f. 51)	71	May 7	8	Greenwich.
295·9	−24·8	19·0	Niesten III. 6	77	Oct. 13	7 15	Brussels.
297·0	−22·6	24·5	Green 11	77	Sept. 10	11 20	Greenwich.
297·1	−22·6	24·6	Boeddicker II. 1	77	Sept. 8	11 1·5	Berlin.
298·4	−22·5	24·7	Boeddicker I. 2	77	Sept. 6	9 54·6	Berlin.
299·5	+22·0	15·7	Knobel 14	73	May 23	9 0	Greenwich.
300·3	+22·0	15·6	Burton (*Ar.* f. 11)	73	May 24	9	Dublin.
304·4	−22·6	24·6	Boeddicker II. 2	77	Sept. 8	11 31·4	Berlin.
304·4	−25·3	18·0	Niesten IV. 1	77	Oct. 18	11	Brussels.
304·5	+15·8	15·4	Terby f. 15	73	Apr. 13	9 50—10 30	Brussels.
309·8	−22·6	24·6	Dreyer 2	77	Sept. 8	11 0	Greenwich.
310·8	+22·2	15·5	Green No. 4 (*Ar.* f. 15)	73	May 25	11	Greenwich.

Areographical Long. and Lat. of the Centre of the Disk.		Diameter.		Times assigned to the Sketches.			
°	°	″				h m h m	
?311·4	−22·6	24·6	Boeddicker III. 1	1877	Sept. 8	*circa* 12	Berlin.
312·4	−24·8	19·0	Terby 15	77	Oct. 13	8 15— 8 30	Brussels.
314·0	−22·6	24·5	Boeddicker III. 2	77	Sept. 10	13 23·3	Berlin.
314·5	−24·9	18·8	Lohse 11	77	Oct. 14	9 45	Berlin.
314·8	+22·5	15·2	Trouvelot 4	73	May 29	9 0	Cambridge, Mass.
316·4	+21·7	15·9	Terby f. 24	73	May 20	8 30— 8 45	Brussels.
317·6	−25·0	18·6	Niesten III. 9	77	Oct. 15	10	Brussels.
?322·0	−22·6	24·6	Dreyer 3	77	Sept. 8	?11 50 Time uncert.	Greenwich.
322·2	−21·3	22·9	Knott (*Ar.* f. 26)	62	Sept. 23	8 30	Greenwich.
324·2	+25·4	14·2	Webb (*Ar.* f. 13)	71	Mar. 29	11	Greenwich.
325·1	−25·7	18·5	Knott (*Ar.* f. 9)	62	Nov. 3	9	Greenwich.
326·3	−22·6	24·6	Cruls VIII. 2	77	Sept. 8	9 15	Rio de Janeiro.
326·8	+25·4	14·2	Vogel No. 9	71	Mar. 29	11 51	Bothkamp.
327·5	+21·1	16·2	Schmidt (*Ar.* f. 17)	73	May 16	8 15	Athens.
330·8	−22·5	24·6	Dreyer 1	77	Sept. 7	11 50	Greenwich.
331·7	−22·6	24·6	Green	77	Sept. 8	12 30	Greenwich.
332·3	−22·6	24·5	Boeddicker IV. 1	77	Sept. 10	14 38·1	Berlin.
334·5	+22·3	15·4	Trouvelot 3	73	May 23	8 30	Cambridge, Mass.
337·7	+25·2	14·3	Lohse No. 8	71	Mar. 25	10 13	Bothkamp.
340·9	+27·1	11·6	Wilson (*Ar.* f. 14)	71	May 4	8	Greenwich.

343·7	−24·5	19·8	Niesten III. 4	77	Oct.	9	8	Brussels.
344·9	+21·9	15·8	Knobel 13	73	May	22	11 30	Greenwich.
345·2	−24·6	19·6	Lohse 10	77	Oct.	10	9 20	Berlin.
345·5	−24·5	19·8	Terby 14	77	Oct.	9	8 — 8 15	Brussels.
346·0	−22·6	24·6	Weinek 1	77	Sept.	8	14·3	Leipzig.
347·1	−22·5	24·7	Boeddicker I. 1	77	Sept.	5	12 38·1	Berlin.
349·5	−22·7	24·3	Cruls X. 2	77	Sept.	12	13 15 *corr.*	Rio de Janeiro.
350·8	−24·8	19·0	Niesten III. 7	77	Oct.	13	11	Brussels.
353·2	−24·8	19·0	Cruls XIII. 2	77	Oct.	13	8 0	Rio de Janeiro.
354·6	+21·5	16·0	Knobel 11	73	May	19	10 20	Greenwich.
355·8	−24·6	19·6	Dreyer 12	77	Oct.	10	9 10	Greenwich.

When, some years hence, the direction of the axis of *Mars* shall have become better determined than it is at present, these lists will have to be recomputed, and, with the addition of a select number of the old sketches (and, perhaps, with the omission of some of the modern ones), united into one general list. If it is thought desirable, the assumed First Meridian may then be shifted so as to pass through some definite point of Maedler's spot *a*. Owing to some discrepancies in the sketches of 1862, the assumed meridian 0° passes a little to the west of the middle of spot *a*; but the true amount of the difference can only be determined when proper observations shall have been forthcoming.

Observations of Brorsen's Comet, February and March 1879.

By J. Tebbutt, Esq.

Owing to an extraordinary succession of cloudy evenings I have succeeded in obtaining only two determinations of position of Brorsen's comet since the date of my last communication. Each position depends on two ring-comparisons with the $4\frac{1}{2}$-inch Equatoreal. There was no condensation of light in the comet, so that the observation errors will probably prove rather large.

Windsor Mean Time 1879.	App. R.A. of Comet.	Log. for Parallax.	App. N.P.D. of Comet.	Log. for Parallax.	Comp. Star.
d h m s	h m s	+	° ′ ″	+	
Feb. 26 7 49 46	0 49 57·60	8·7454	99 34 50·8	9·7165	B.A.C. 248
Mar. 11 7 18 41	1 30 29·42	8·7299	90 1 17·9	9·7407	Lalande 3045

The adopted mean places of the stars of comparison for 1879·0 and the apparent places for the dates of observation are:—

Comparison Star.	Mean R.A.	App. R.A.	Mean N.P.D.	App. N.P.D.
	h m s	s	° ′ ″	″
B.A.C. 248	0 48 11·45	11·86	99 23 49·5	48·6
Lalande 3045	1 33 12·92	13·37	90 0 7·8	4·7

The comparisons on February 26 are corrected for proper motion and refraction, but the latter correction for the comparisons of March 11 will hardly be appreciable.

Observatory, Windsor, N.S.W.,
1879, *March* 26.

A New Method of controlling the Driving Clock of an Equatoreal.

By R. C. Johnson, Esq.

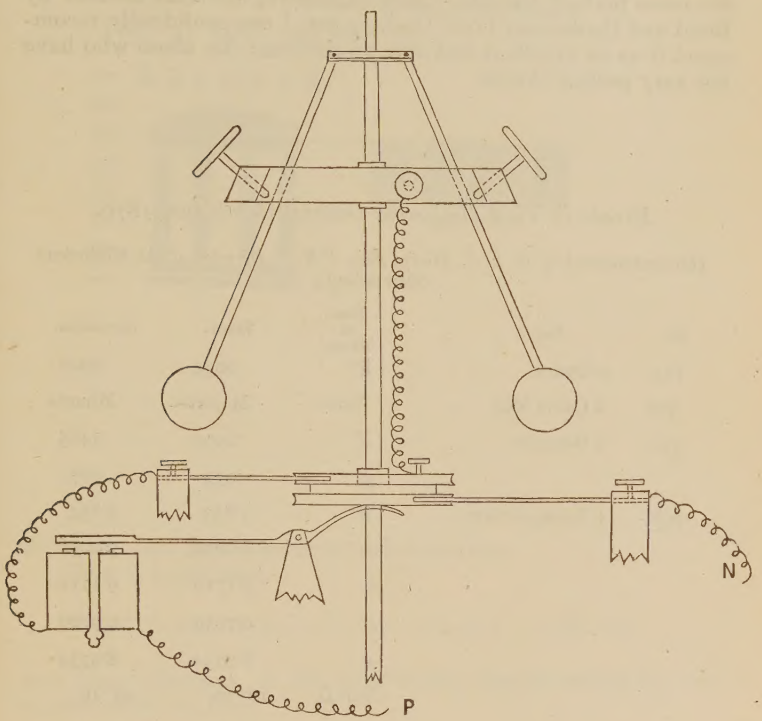
This is a description of an automatic brake to be applied to a rapidly moving part of the mechanism of an ordinary equatoreal Driving Clock whenever the normal velocity is exceeded.

As applied to my Driving Clock belonging to a $9\frac{1}{4}$-inch Reflector, it consists of the following parts:—A frame of brass, slotted so as to enclose, without touching, the centrifugal arms of the governor, is fixed to the upright axis to which these arms are attached, but is insulated from it: in the frame are two brass screws,* which can be regulated so as just to be in contact with the arms when the proper speed has been attained. Two metallic wheels are fixed to the lower part of the axis, the upper of which

* Only one of these need be in use.

is insulated from it, but connected by a wire with the frame in the upper part, and the lower one is in metallic contact with the axis. Two light springs are in contact with these wheels, and an electro-magnetic brake (as shown by the figure) is balanced so as to press lightly on the base of the lower wheel when no current is passing.

Four to eight small-sized Leclanché cells suffice to actuate this brake.



No current can pass until the weighted arms fly out so as to touch one of the screws in the upper frame; electrical contact is then made, the brake acts instantly, and immediately the arms drop an infinitesimal distance and contact is broken: this intermittent action goes on rapidly, and results in an extremely steady speed, the oscillations being so minute that they are barely perceptible, even with a magnifying power of 500 diameters.

I use a driving weight of 40 lb., and find that when this brake is in action 22 lb. can be added without affecting the rate; and I feel sure that by varying the leverage of the brake, or the battery power, or the size of the electro-magnet, that the weight might just as easily be doubled.

This control was devised in order to obviate the necessity of accurately balancing the telescope (when using a spectroscope, for instance) and to prevent the slackening of the clock's speed, which is frequently caused by a rapid fall in the temperature.

The idea was suggested by a description of a control for machinery invented by Mons. Marcel Deprez,* which, however, is exactly the converse of the plan here described.

Of course I do not intend this simple method to compete with the more perfect, but also more expensive, methods devised by Bond and Cooke and Lord Lindsay, but I can confidently recommend it as an excellent and cheap substitute for those who have not very perfect clocks.

Errata in First Melbourne General Catalogue, 1870.

(*Communicated by R. L. J. Ellery, Esq., F.R.S., Director of the Melbourne Observatory.*)

No.	Name.	Place of Error.	Error.	Correction.
194	γ Hydræ	P″	·0034	·0008
358	α Canis Min.	Name	Majoris	Minoris
552	η Octantis	P′	·2099	·1485
		p'	·033	·007
796	κ Trianguli Aus.	P	5·833	5·834
		a	9·0054	9·0056
		b	9·1710	9·1712
		c	0·7659	0·7660
		d	8·9732	8·9734
		N.P.D.	10″·10	40″·10
		m	(−0·25)	(+0·01)
		a'	9·7822	9·7823
895	Octantis, B.A.C. 5976	P′	·8356	·7589
947	α Lyræ	N.P.D.	30′	20′
1067	Octantis, B.A.C. 7020	P	102·365	102·372

1879, *April* 16.

* See *English Mechanic* for March 14, 1879.